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VOL. IX. No. 5 SEPTEMBER, 1902

THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA.

COMMUNICATED BY PROFESSOR GEORGE M. STRATTON.

III. VISIBLE MOTION AND THE SPACE THRESHOLD.

BY PROFESSOR GEORGE M. STRATTON.

For some time it has been argued that the perception of motion has no immediate connection with the discrimination of positions in space. It is held that the two processes are psychologically independent, and that we become aware of motion by a direct and simple sense of the motion itself, and not by appreciating that the object occupies distinct localities.

This paradox is supported in part by the fact that a movement in the out-lying portions of the visual field can be readily seen when the movement has a far less extent than is required to give to two motionless objects a perceptible difference of position,—a fact apparently first observed by Exner. To state the matter in the words of Professor James: "One's fingers when cast upon the peripheral portions of the retina cannot be counted—that is to say, the five retinal tracts which they occupy are not distinctly apprehended by the mind as five separate positions in space—and yet the slightest *movement* of the fingers is most vividly perceived as movement and nothing else. It is thus certain that our sense of movement, being so much more delicate than our sense of position, cannot possibly be derived from it. * * * Movement is a primitive form of sensibility."[1]

[1] 'Principles of Psychology,' II., 172.

One may rightly have some antecedent hesitancy, I believe, in regard to James' interpretation. Even admitting the facts he offers, one need by no means draw his conclusion. For the alleged sense of motion, if it really is a sense of *motion*, brings in a spatial report. The changes of which it makes us aware are preceptibly different from alterations merely of intensity or of color. Even though we may be unable to tell the direction of the motion, the motion itself is a change of position, and is dimly appreciated as such. Instead of saying, then, that the experiments cited are evidence that the sense of movement is so much more delicate than the sense of position, it would be more exact to say that they show that the discrimination of positions during movement is much finer than the discrimination of positions at rest. It is not really an antithesis between 'motion' and 'differences of position,' but between differences of position under two contrasting sets of conditions, motion being but a special mode of testing our power of local discrimination. The truth seems to be that there are various ways of measuring this power—among others, by the simultaneous presentation of two lights in different places, by their successive presentation, or by a continuous movement of a single light from one position to another. We have no right to assume (as Professor James seems to do) that the first of these methods is the only one that gives the true space-threshold, and that the results of the third, if finer, are indications of a process different in kind. The second method also gives finer results than the first, and yet no one, so far as I know, has thought that the finer space results obtained by successive stimulation implied some special and primitive form of sensibility different from that which is involved in discriminating simultaneous impressions. Why, then, should we jump to this conclusion when the conditions of space-perception are only slightly altered farther, making the successive stimulation spatially continuous instead of discrete?

Admitting the facts adduced, then it by no means follows that we have a primitive sense of movement, independent of spatial discrimination. But at least so far as space is concerned, the facts themselves are not unquestionable. Some experiments

by Stern, reported in 1894,[1] already raised a doubt here. Stern found that when retinal irradiation was decidedly reduced, the shortest perceptible movement was not appreciably less than the space-threshold as determined without movement. He consequently inferred that there was no ground for assuming a specific and unique sense of movement. But the test of motionless space-discrimination used by Stern is itself perhaps not fully convincing. He departed from the method employed by Helmholtz and others — the method by parallel lines brought closer and closer together until they almost fused — and, instead, took as the threshold that width of a single dark line that was just doubtfully perceptible against a light background. The assumption here seems to be that the line is a gap or interruption of the white surface, and implies a local discrimination of the two borders of white against the line which divides them. But if we regard black as a positive impression, as it would seem we must, there would appear to be no reason why a consistent development of this method would not require us to accept the apparent diameter of a just perceptible fixed star as a still more accurate measure of local discrimination. The width of the dark line in Stern's experiments is probably of importance only in a secondary way, by affecting the intensity of the impression of black. This suspicion is strengthened by the fact that with some slight improvements in the conditions of observation in our laboratory, while still remaining true to the general principle of Stern's method, the threshold takes an astonishing drop. One of my students, Mr. Gilbertson, finds that the width of the black line visible against white, instead of being 15″ angular measure, as Stern found under his conditions, may be even less than 2.5″.

For this reason it seemed well to try some experiments in which the threshold of motion and that of local discrimination might be compared without so much doubt as to the really spatial character of this local discrimination. The experiments here reported fall into two groups, the first with indirect, the second with foveal vision.

[1] Die Wahrnehmung von Bewegungen vermittelst des Auges, *Zeitsch. f. Psychol. u. Physiol. d. Sinnesorg.*, VII., 321.

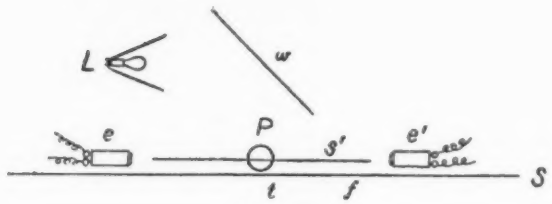

FIG. 1.

I. *Experiments with Indirect Vision.*—The observer, in Fig. 1, sat in a half-darkened room and judged the character of the light-stimulus seen through a narrow vertical slit *t* about ½ mm. wide in a screen *S* before him. The fixation point *f* was 5° to the right of the slit in one set of experiments and 30° to the right in the other, the distance from the observer to the screen being in these two cases 2 m. and 1 m., respectively. When the aim was to determine the threshold of motion, a bright point of light moved from a fixed position upward or downward in the slit, while the local discrimination was tested by two separate motionless points of light which appeared the one above the other and in immediate succession. The extent of movement, in the one case, and of spatial separation in the other, were of course accurately varied to determine the threshold.

This variation, along with a constancy of those conditions that should remain constant, was brought about by an arrangement behind the screen, consisting chiefly of a pendulum *P* carrying a smaller slitted screen *S'*, which in swinging past the slit *t* allowed light to pass to the observer from a white surface *w* evenly illuminated by an electric lamp, *L*. The pendulum was so controlled by the electromagnets *e* and *e'* as to give a single swing from a fixed point 10° from the vertical and be caught at the end of its course on the other side. The back-swing in preparation for the next experiment was concealed from the observer. The length of the pendulum was regulated to give a single oscillation in one second.

In Fig. 2 there is represented in diagram the screen *S'* that was carried by the pendulum when the aim was to give the observer a moving point of light. The main feature of this

screen was a movable circular disc D, from whose center, c, there passed to the periphery a slit cp, ½ mm. in width, forming an arc of the circle whose radius was the distance from C to the point of suspension of the pendulum (70 cm.). When the

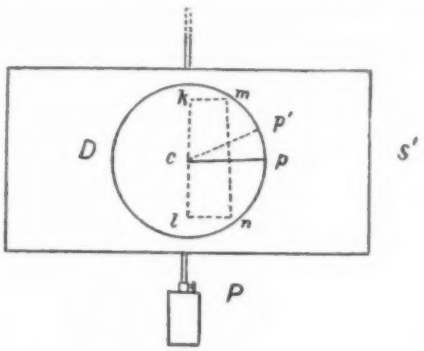

FIG. 2.

disc was so set that the center of this arc coincided with the point of suspension of the pendulum, the point of light which came to the observer as this arc-slit swung past the vertical slit t in the stationary screen S in Fig. 1 had no apparent motion whatever. A change of the disc's position, however, so that the slit took the position cp', would cause an apparent upward motion of the point of light, the extent of which in angular measure, from the point of view of the observer, could readily be calculated. A scale in terms of such angular measurement was placed along the circumference of the disc so that this could be set for any extent of motion desired. But since it seemed best to keep the duration of the movement exactly the same as the duration of the stimulus in the corresponding set of experiments with motionless points, this constancy, in spite of the varying positions of the disc, was maintained by the fact that the screen S' was continuous behind the disc D with the exception of an opening $klmn$, whose boundaries kl and mn were radii of the circle mentioned above, the center of which was the point of suspension of the pendulum. It need only be added that this screen was wide enough to conceal the surface w in Fig. 1, when the pendulum was at either limit of its excursion.

For the experiments on the discrimination of position without movement, a different screen was substituted upon the pendulum, yet with a similar arc-slit similarly centered. But in this case, instead of revolving the slit about a central point, one half of the slit could be shifted slightly up or down, along the radius of the circle of which the arc was a portion, as in

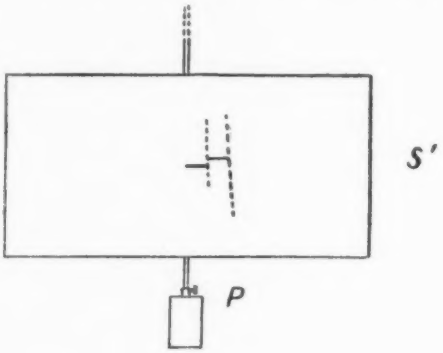

FIG. 3.

Fig. 3. As the pendulum made its single swing, there thus appeared to the observer in immediate succession two separate points of light, the amount of whose separation could be accurately varied by the experimenter.

In the preliminary experiments, the well-known method of 'minimal changes' was employed, but the observers soon showed themselves so much influenced by expectation that no reliable determination of the thresholds seemed possible by this means. The method of 'serial groups,' hereinafter described, was found more satisfactory. The adoption of successive stimuli to measure the local discrimination without movement, was for the purpose of avoiding some of the worst difficulties from irradiation. Two points of light when presented simultaneously will often produce a blur that is perceptibly extended (and consequently spatial), and yet two separate nuclei of light are not distinguished within this vague extent. According to the usual method of interpreting the results, we should here be below the space-threshold, although, of course, we are not. What Tawney has already justly insisted on with regard to

tactual impressions,[1] that the perception of two points is not a true measure of the threshold, might consequently be repeated with respect to vision. The fusion of the two points is largely obviated by the use of successive stimuli so that the intensity of the first impression is perceptibly lessened by the time the second is at its height.

But the substitution of successive for simultaneous impressions brings what some might consider a vitiation of the results. The two points of light, even when well separated, give the psychological impression of continuous motion; the light seems to leap from the position of the first point to that of the second. At the beginning it was thought possible to separate those judgments in which an apparent motion entered, and treat them as a questionable group by themselves. But when once the suggestion of motion becomes fairly lodged in the observer's mind, it occurs so persistently and over such a range that this attempt was renounced. Since there is no objective movement here, however, and the whole thing is really a matter of suggestion pure and simple, and seems to me to imply an underlying space-discrimination as the basis of the suggestion itself (as I have urged at the beginning of this paper), the judgments with subjective motion have been freely employed in computing the thresholds. Those to whom these reasons do not seem sufficient may have no such scruples over the second group of experiments on foveal vision; not even suggested movement there enters into the judgment.

The results of the experiments with indirect vision were as follows:

Observer.	Angle of Observation.	Number of Determinations.	Discrim. of Position.		Perception of Motion.	
			Threshold.	M. V.	Threshold.	M. V.
A	5°	6	9.2′	1.6′	10.4′	1.4′
	30°	5	29.0′	5.2′	29.0′	3.2′
Bd	5°	6	7.9′	.7′	8.3′	1.9′
	30°	3	18.3′	2.2′	21.7′	5.6′
Bl	5°	3	7.5′	3.3′	6.6′	2.2′
D	30°	4	63.7′	8.7′	70.0′	10.0′

[1] 'The Perception of Two Points Not the Space Threshold,' PSYCHOL. REVIEW, II., 585.

The table indicates that where the conditions are practically equal, the perception of movement has no advantage over the discrimination of position without objective movement. So far as indirect vision is concerned, the theory that the two processes are psychologically independent here finds no support.

II. *Experiments with Direct Vision.*—The second group of experiments dealt with the same problem as the preceding, with a change merely in the method of investigation and with a different portion of the retina—the foveal instead of the excentric region of sight. On account of the extraordinary nicety of our space discrimination under these conditions, the observer was placed in a distant building where there was an unobstructed view of one of the laboratory windows, the distance of his station from the object to be observed amounting by accurate survey to 120 m. The observer faced the north, and the portion of the laboratory towards which he looked sent back no direct glare from the sun; preliminary experiments showed the need of regarding these things. The distance of the observer's station, it is true, was inconvenient in many ways, but this was more than offset by the greater ease of observation. The naked eye could now be employed, and all the difficulty was avoided that comes from the use of a reducing lens, such as Stern found necessary. The fact that the threshold of movement easily ran down to about one half of what he reports shows the advantage of these conditions. For the perception of motion a strip of white bristol-board, 8 mm. wide by 50 cm. long, was mounted vertically on a dead-black frame which could be moved horizontally before a larger dead-black background. From the observer's station the frame itself was invisible, aud all that one saw was the white line on the large black field. The movement of the line was controlled by runners and guides and adjustable stops on the frame, so that the motion was kept horizontal and its excursion varied by steps of 1 mm. The experimenter moved the frame by hand, and during any single observation kept the extent of the movement constant, and continued the oscillations from the moment just before the exposure of the line until the observer had signalled his judgment. In order to insure full justice to this side of the investigation, the *rate* of movement

had, also, of course, to be taken into account; otherwise some velocity might be selected that would not be the most favorable to perception. For this reason the experiments on motion were subdivided into five groups, with a rhythm, respectively, of 60, 100, 180, 300, and 450 single swings to the minute. The rates were maintained with reasonable accuracy by the aid of an adjustable pendulum invisible to the subject of the experiment.

For the corresponding set of experiments on the discrimination of position without movement the conditions were in all respects the same, except that for the moving frame with its single vertical line two vertical lines end-to-end against the dark background were substituted. Of these two lines the lower was fixed, while the upper was capable of being shifted from continuity with the lower line to any position to the right or left, remaining throughout parallel, however, to its original position. The actual settings varied by steps of 1 mm., and the observer had simply to judge whether the two lines at any given setting were continuous, or in what respect they spatially differed.[1]

The results of the two sets of experiments under these conditions were as follows:

DISCRIMINATION OF POSITION WITHOUT MOVEMENT.
Thresholds in mm.

Observer S.		Observer Y.	
Left.	Right.	Left.	Right.
4	4	6	3
7	5	4	6
3	5	5	6
7	5	6	6
3	3	7	4
4	6		
Av. 4.3	4.3	5.6	5.0

The thresholds for the discrimination of position without movement are slightly higher than those obtained in my previous study already referred to, but the difference is not such as to call for any special comment. The thresholds for the perception of movement, as affected by the rate of move-

[1] For a more detailed account of this method, see my 'New Determination of the Minimum Visibile and its Bearing on Localization and Binocular Depth,' PSYCHOL. REVIEW, VII., 429.

PERCEPTION OF MOVEMENT.
Thresholds in mm.

Single Oscillations per Minute.	60	100	180	300	450
Observer S.	11	6	4	4	4
	16	4	4	4	5
	14	7	4	4	4
	8	5	4	4	6
Av.	12	5.5	4	4	4.75
Observer Y.	25	14	7	4	7
	15	14	5	6	7
	18	8	6	6	6
	16	12	6	5	6
Av.	18.5	12	6	5.2	6.5

ment, take a somewhat different course from those reported by Stern.[1] This investigator worked with rhythms of 144, 84 and 72 vibrations to the second, and found *merkwürdigerweise* (as he says) that the slowest movement was the one most distinctly perceived. In my own results, it will be seen that the lower rates, those of 60 and 100, are markedly unfavorable to perception, with a decided improvement as we pass to 180 and 300, the threshold again rising slightly as the rate is still farther increased. Virtue here seems again to lie in the mean. The lower rates require a considerable excursion before they can be distinguished from rest, while the most rapid oscillations tend to produce a mere indistinctness rather than a perceptible swaying of the object. I am quite at a loss to account for our divergent results here, unless it be that by the word '*Schwingungen*,' which Professor Stern uses without modification, he means '*double* vibrations.' In this event his lowest rate would be about midway between my 100 and 180, and he would have caught only the dip of the threshold as it comes out of the region where the rapidity of the impressions produces blur, without his rates becoming slow enough to show the upward trend of the threshold again farther on. My own subjects, like his, never knew beforehand the rate that was being used, so the discrepancy cannot be explained in this way. Possibly the difference is in some way connected with the fact that his observers had to look through the lens of a microscope.

[1] *Zeitschrift für Psychologie u. Physiol. d. Sinnesorg.*, VII., 347.

For observer *S*, the most favorable rates of motion give a threshold of 4 mm. (about 6.8″ angular measure) as against 4.3 mm. (about 7.3″) for the discrimination of place—a net advantage, for motion, of .3 mm. or .5″ of arc, on the average. If, however, we were to disregard averages and look at the individual determinations, it would appear that the space discrimination occurred as low as 3 mm. (5.1″ of arc); while in no case does motion become perceptible below 4 mm. For observer *Y*, the mean threshold for position without movement is 5.3 mm. (9″); while the most favorable rate of motion gives 5.2 mm. (8.8″) on the average. Here again the minimum thresholds for position are below the smallest for movement. Thus, from one point of view, motion has a slight advantage over discrimination without movement, which advantage, however, is reversed when the numbers are differently regarded. But in either case the difference is too slight to serve as a sufficient basis for entirely differentiating the psychological processes involved.

The results here, then, are in substantial agreement with those obtained in indirect vision by an independent method. The doctrine that visual motion is a primitive form of sensibility independent of local discrimination finds no experimental warrant. The perception of motion seems to be nothing more nor less than the perception that a sensation is changing its space relations, the motion itself furnishing a decidedly favorable, but by no means unique, set of conditions for appreciating such differences of space relationship. This does not imply that the detection of movement always involves a deliberate comparison of positions; for the discrimination often undoubtedly occurs at a single psychic stroke. But even this apparently simple stroke is really a complex act. It implies a relational activity of the mind which interprets and gives character (crude and confused though it be) to the incoming sensations, so that they are no longer blank impressions, but are impressions which mean for us *movement*. The experiments thus go to support the view that a fact of space can never be conveyed to the mind in the form of a pure sensation divested of all relationship.

IV. The Method of Serial Groups.

BY PROFESSOR GEORGE M. STRATTON.

In the practical conduct of the laboratory one frequently feels the shortcomings of the method of 'minimal changes.' It is undoubtedly the best all-around mode of procedure yet devised, but in certain cases where the conditions are exceptional it may leave one quite *im Stich*. This is especially true when one is dealing with minimal impressions, where suggestion is apt to find such free play; the observer may continue to notice a sensation when the stimulus has become suspiciously weak—in fact when no stimulus at all is applied. Thus, with certain excellent though suggestible subjects, I have found it impossible to determine by the method of minimal changes, pure and simple, the least extent of visible motion that could be perceived as motion. The subjects persisted in seeing the light move on every occasion, whether there was any actual movement or not. The control of the answers, by requiring that the observer shall tell correctly some additional feature of the impression—tell, say, the direction of the movement, or, if the experiments be on the least perceptible change of pressure, tell whether the pressure becomes heavier or lighter — may in some instances be helpful. But often this check will hide the very facts that one wishes to ascertain — the point at which the subject perceives motion and yet is uncertain of the direction, or notice change of pressure without being able to say whether the weight has grown greater or less.

The usual resort in this event is either to a so-called 'catch experiment' the *Vexirversuch*, where no stimulus at all is given, or to the method of right and wrong cases. The latter, making use as it does of the law of probability, not only requires an extremely large number of observations, but there is usually needed considerable preliminary and irregular experimentation in order to discover the conditions that will give a suitable proportion of right and wrong answers. The *Vexirversuch*, on the other hand, has never been systematized, and as it is usually introduced in an irregular fashion within the

method of minimal changes, it is apt to disturb the even tenor of the research, and disconcert the observer whenever he gets a hint of what is being done.

In the method of serial groups here proposed, the attempt is made to legitimate the 'catch' experiment, to introduce it as a continuous and regular element of the procedure, while securing certain advantages both of the method of minimal changes and of the method of right and wrong cases. To give a concrete illustration, suppose the following groups of experiments be carried out to determine the just perceptible extent of movement by sight, under the conditions described in the second part of the preceding paper:

Group I.			Group II.		
Exp. No.	Amount of Motion.	Judgment.	Exp. No.	Amount of Motion.	Judgment.
1	0	no motion.	11	0	no motion.
2	7 mm.	motion.	12	0	" "
3	7 mm.	"	13	6 mm.	motion.
4	7 mm.	no motion X.	14	6 mm.	"
5	0	" "	15	0	no motion.
6	0	" "	16	6 mm.	motion.
7	7 mm.	motion.	17	0	no motion.
8	0	no motion.	18	6 mm.	motion.
9	0	" "	19	0	slight motion X.
10	7 mm.	motion.	20	6 mm.	motion.

Group III.			Group IV.		
Exp. No.	Amount of Motion.	Judgment.	Exp. No.	Amount of Motion.	Judgment.
21	5 mm.	no motion X.	31	4 mm.	very slight motion.
22	0	" "	32	0	no motion.
23	5 mm.	motion.	33	0	" "
24	0	no motion.	34	0	very slight motion X.
25	5 mm.	" " X.	35	4 mm.	no motion X.
26	0	" "	36	0	" "
27	5 mm.	motion.	37	4 mm.	motion.
28	0	no motion.	38	4 mm.	"
29	0	" "	39	0	no motion.
30	5 mm.	motion.	40	4 mm.	" " X.

The X shows the errors in any group, and from these the threshold may be determined according to any proportion of correct and incorrect answers that may be chosen. In my own computations that group has been taken as giving the threshold beyond which less than eight out of the ten judgments are right. But a detail like this, as well as the exact number of

experiments that may best form a 'group,' might well be considered as subject to revision in the light of farther experience, and not as an essential part of the method. The essence of the matter is simply that there should be groups of experiments arranged in a regular series, the amount of positive stimulus, as one passes from group to group, being graduated according to the principle of the method of minimal changes; while within the limits of any one group a constant stimulus is irregularly alternated with cases where the stimulus is zero, thus uniting in the single group the basal principle of the method of right and wrong cases and that of the *Vexirversuch*. This may seem provokingly eclectic, but it is not exactly that; the different elements make an organic union, and not a mere patchwork. There is simply an attempt to make systematic what experimenters have frequently found themselves compelled to do in a casual and uncritical way.

One may perhaps repeat that this method is not proposed as a general substitute for the classic ones in use. It is well, however, to multiply our tools so that the best may be selected for the special work in hand. And this one has been found good for certain purposes, especially where suggestion plays a prominent rôle. The observer may here know from the very beginning the general method of procedure; he may know that zero-cases are to be irregularly alternated with those of positive stimulation, and his expectation is therefore less 'set' and influential. The zero-cases no longer come in as a kind of indignity upon the observer, as if his word were being questioned. The check here, because of its constancy, ceases to excite any feeling. The procedure, moreover, has the virtue of the method of minimal changes, in that the threshold is ascertained empirically, by actually crossing it. And while the principle of right and wrong cases is employed, with the powerful control which that always brings, yet there is no introduction of the intricate calculus of probability and a certain darkness that always shadows its results. It is true that the application of the method of serial groups is in a certain sense cumbersome, as compared with the method of minimal changes, since in a given time fewer determinations of the threshold can be obtained. But

with suitable rests between the 'groups,' there is no need of there being greater fatigue to the observer in the one case than in the other; and while the determinations may be fewer for the time expended, yet in most cases I have found that they more than make up in weight what they lack in number.

V. The Effect of Subdivisions on the Visual Estimate of Time.

BY MABEL LORENA NELSON.

It has been found by Dr. Ernst Meumann and others that the estimate of small time-intervals is influenced by the number of stimuli that fall within the interval. In the space illusion of sight, a single division of the standard will cause it to be underestimated, while more divisions will cause an overestimation; in touch, the effect of subdivisions depends on the absolute length of the standard.[1]

My object, in the following experiments, was to determine the effect of single and multiple divisions of the standard on times of longer duration than those investigated by Dr. Meumann, and to discover if there existed a temporal illusion comparable to the space illusions of sight and touch.

In Dr. Meumann's investigation of time intervals, he compares an 'empty' time — one bounded by two impressions — with times 'filled' with either three, five, six, nine or twelve impressions, inclusive of the terminal stimuli. His results[2] are, that for times from one tenth of a second to about four seconds, when the filled time comes first, the error in estimating is constantly positive — while for longer times the error is negative.

This seems to indicate that the effect of the filling is positive for the short times, and negative for the longer. The error found by Dr. Meumann is, however, not due to the filling alone, but is the result of two factors. It is generally conceded that even when two empty times are compared, there is a similar constant error, positive for short times, negative for longer.

[1] See the paper by Miss Alice Robertson, on "'Geometric-Optical' Illusions in Touch'" to be published subsequently.

[2] 'Beiträge zur Psychologie des Zeitbewusstseins,' *Phil. Studien*, XII., p. 127.

That there is a difference other than this constant error which must be attributed to the filling, Dr. Meumann shows—for, in those cases where the order is reversed, the empty time coming first, the sign of the error is also reversed — but the quantity of the error due to the filling alone he does not show, as these two factors are not quantitatively separated.

The longest period chosen by Dr. Meumann was nine seconds. The following experiments were taken to determine what effect the filling would have on longer periods, durations of several minutes.

The intervals chosen were one half, one, two, four, six and ten minutes; the filling, sensations of light.

Under each interval two sets of estimates were taken. The first, where the standard and compared times were both empty (marked *E*–*E* in the tables) was taken to determine the constant error due to the mere sequence of the two intervals. In the second set one of the times was always empty and the other filled (*E*–*F* and *F*–*E* in the tables). Any difference found between the estimates of the two sets, for a given interval, must be due to the filling.

The results as given in the tables are computed from five estimates under each interval for the empty time, and five for the filled. The average of the estimates is given; the difference between this and the standard interval, expressed as a per cent. of the standard; and the mean variation from the average estimate, expressed as a per cent. of the average. A second basis of comparison is the median of the five estimates and its difference, as a percentage, from the standard interval.

The effect due to the filling for each interval is found by subtracting the constant error, when both standard and compared times are empty, from the error in estimating when one of the times is filled. When the difference due to the filling in Tables I. and II. has a positive sign, it must be taken to mean that the filled time seemed shorter than an empty one of the same length. In Tables III., IV. and V., however, the order is reversed, the filled time being taken as the standard — a positive error here would indicate that the filled time seemed longer than the empty.

The method of taking the experiments was as follows: The subject sat in a darkened room before a screen and saw through an aperture in the screen, 5 mm. wide by 10 mm. in height, flashes of light through a noiseless pendulum behind. A flash of light marked the beginning of an empty time, a second flash its end. During the filled time the subject saw a flash of light every half second. There was in every experiment a pause of two seconds between the closing flash of the standard and the first flash of the compared time. The end of the compared time was marked off by a word from the subject when a time had elapsed which seemed to him equal to the standard.

In the first group of experiments the standard time was always empty. Two subjects were taken, *D* and *R*; the results are found in Tables I. and II. The great difficulty my subjects found, in the long intervals, in keeping their attention on the length of the standard, made it necessary to give them some idea of the interval to be used. Accordingly they were told whether the interval would be short (one half and one minute were called short), moderate (two and four minutes), or long (six and ten minutes), and whether the compared time would be filled or empty. This was, of course, in some respects a disadvantage, as it perhaps affected the lengths of the estimates, but as my object was to compare the estimates of a filled and an empty interval of time, the results are not invalidated by this guidance, as it was given alike in both sets.

In order that there might be no constant effect due to contrast, the order of using the different lengths as standards was not fixed, but was determined by chance. The time occupied by my work was never more than one hour at a time.

TABLE I.

SUBJECT *D*. *E-E*.

Interval.	Av. Estimate.	D ⅌	M.V. ⅌	Median.	D ⅌	M. V. ⅌
½ min.	28.2 secs.	— 6	27.5	34 secs.	+ 13	19.4
1 "	45.4 "	— 24.3	10.7	44 "	— 26.6	10.4
2 "	1 min. 40.6 "	— 16	12.6	1 min. 40 "	— 16.6	12.6
4 "	2 " 58.2 "	— 25.7	30.2	2 " 26 "	— 39.1	33.7
6 "	4 " 56.8 "	— 17	14.7	4 " 43 "	— 26.9	14.4
10 "	7 " 21.4 "	— 26.4	31	7 " 22 "	— 26.6	34.2

E–F.

½ min.	53.4 secs.	+ 78	40.1	43 secs.	+ 43.3	35.3
1 "	1 min. 18.6 "	+ 31	15.4	1 min. 17 "	+ 28.3	23.4
2 "	2 " 6.6 "	+ 5.5	30.4	1 " 55 "	— 4.1	35.1
4 "	4 " 6.8 "	+ 2.8	27.1	4 " 20 "	+ 8.3	27.7
6 "	6 " 7.4 "	+ 2	33.5	6 " 53 "	+ 14.7	27.5
10 "	7 " 1.8 "	— 29.7	18.4	7 " 26 "	— 25.6	16.3

Difference due to Filling.

	Average.	Median.
½ min.	+ 84 per cent.	+ 30 per cent.
1 "	+ 55.3 "	+ 54.9 "
2 "	+ 21.5 "	+ 12.5 "
4 "	+ 28.5 "	+ 47.4 "
6 "	+ 19 "	+ 41.6 "
10 "	— 3.3 "	+ 1 "

The results as given in Table I. show that when the average is taken as the basis of comparison, the effect of the filling on the estimates in the case of *D* is very constant. In the interval of a half minute, a filled time 84 per cent. greater than the empty is taken to be its equal. The effect of the filling seems to decrease as the length of the interval increases, until at ten minutes it is but little or nothing.

TABLE II.

SUBJECT *R.* *E–E.*

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	34.4 secs.	+ 14.6	26.2	34 secs.	+ 13	25.9
1 "	1 min. 12.2 "	+ 20.6	23.1	1 min. 13 "	+ 21.6	23.5
2 "	1 " 37.8 "	— 18.5	12.8	1 " 39 "	— 17.5	17.4
4 "	3 " 12.8 "	— 19.6	16.3	3 " 17 "	— 17.9	14.5
6 "	3 " 50.2 "	— 36	18.3	3 " 11 "	— 46.9	29.1
10 "	5 " 35.1 "	— 44	18	4 " 55 "	— 50.8	15.1

E–F.

½ min.	36.2 secs.	+ 20.6	33.5	40 secs.	+ 33.3	28.5
1 "	57.4 "	— 4.5	27.3	54 "	— 10	27.4
2 "	1 min. 47.4 "	— 10.5	29.8	1 min. 39 "	— 17.5	30.7
4 "	2 " 56.2 "	— 26.5	36.4	3 " 7 "	— 22	33.1
6 "	3 " 45 "	— 37.5	15.4	3 " 33 "	— 40.8	17.3
10 "	5 " 33 "	— 44.5	8.4	5 " 48 "	— 42	7.5

Difference due to Filling.

	Average.	Median.
½ min.	+ 6 per cent.	+20.3 per cent.
1 "	—25.1 "	—31.6 "
2 "	+ 8.5 "	0 "
4 "	— 6.9 "	— 4.1 "
6 "	— 1.5 "	+ 6.1 "
10 "	— .5 "	+ 8.8 "

With *R*, Table II., the filled time seems shorter than the empty in the intervals of one half and two minutes, but for all the other times it seems longer, taking the average as the basis of comparison. The error in the longest intervals is very small and its sign is changed when the median is taken instead of the average.

In continuing the experiments I introduced the filling into the first or standard time, the compared time being always empty. We should, therefore, expect a reversal of sign in the effect due to the filling. I found that after the former practice the subjects were able to hold their attention to the standard time without any foreknowledge as to its length. In this second group of experiments they were consequently ignorant of the character of the interval to be used. In all other respects the work was conducted exactly as before.

TABLE III.

SUBJECT *D*. *E–E*.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	30.8 secs.	+ 2.6	21.3	28 secs.	— 6.6	21.4
1 "	1 min. 10.2 "	+ 17	29.6	53 "	— 11.6	33.3
2 "	2 " 25 "	+ 20.8	16.6	2 mins. 10 "	+ 8.3	10
4 "	3 " 31.6 "	— 11.4	21.2	2 " 47 "	— 34.1	18.9
6 "	4 " 58.9 "	— 16.9	17.6	4 " 50 "	— 19.4	13.4
10 "	9 " 31.4 "	— 4.7	23.8	10 " 40 "	+ 6.6	22.2

F–E.

½ min.	35.1 secs.	+ 17	15.8	33 secs.	+ 10	13.6
1 "	55.4 "	— 7.6	29	46 "	— 26.6	26.5
2 "	1 min. 26.6 "	— 27.8	26.7	1 min. 14 "	— 38.3	22.1
4 "	3 " 53 "	— 2.9	18.9	4 " 19 "	+ 7.9	15.9
6 "	4 " 43.6 "	— 18.4	20.1	4 " 22 "	— 27.2	18.2
10 "	9 " 28.2 "	— 5.3	21.7	10 " 1 "	0	19.5

Difference due to Filling.

	Average.	Median.
½ min.	+ 14.4 per cent.	+ 16.6 per cent.
1 "	— 24.6 "	— 15 "
2 "	— 48.6 "	— 46.6 "
4 "	+ 8.5 "	+ 42 "
6 "	— 1.5 "	— 7.8 "
10 "	— .6 "	— 6.6 "

The results for *D*, Table III., show that with two exceptions (one half and four minutes) the difference due to the filling has

a negative sign, where in Table I. it was positive, indicating that in general the filled time seemed shorter than the empty.

TABLE IV.

SUBJECT *R*. *E–E*.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	28.2 secs.	— 6	27.7	23.5 secs.	—21.6	29.3
1 "	1 min. 10.8 "	+ 18	23.4	1 min. "	0	24.1
2 "	1 " 33.2 "	—25.5	24.3	1 " 28 "	—26.6	24.5
4 "	2 " 20.5 "	—41.5	25	2 " 14 "	—44.1	16.5
6 "	3 " 41.4 "	—38.5	19.2	3 " 56 "	—34.4	16.7
10 "	6 " 32.9 "	—34.5	25.7	5 " 54.5 "	—40.9	23.1

F–E.

½ min.	31.4 secs.	+ 4.6	30.3	37 secs.	+23.3	22.7
1 "	1 min. 4.7 "	+ 7.8	7.4	1 min. 2 "	+ 1.6	6.9
2 "	2 " 19.4 "	+16.1	29.2	2 " 20 "	+ 1.6	21.8
4 "	2 " 57.6 "	—26	28.2	2 " 34 "	—30.5	29.3
6 "	5 " 52 "	— 3.3	11.6	6 " 1 "	+ 0	10.7
10 "	6 " 15.4 "	—34.1	19.4	5 " 49 "	—41.8	19.3

Difference due to Filling.

	Average.	Median.
½ min.	+ 10.6 per cent.	+ 44.9 per cent.
1 "	— 10.2 "	+ 1.6 "
2 "	+ 41.6 "	+ 28.2 "
4 "	+ 15 "	+ 13.5 "
6 "	+ 35.2 "	+ 34.4 "
10 "	+ .4 "	— .9 "

Subject *R*, in Table IV., shows a positive difference, with the exception of the interval of one minute, where the median and average give conflicting results, and of the interval of ten minutes, where there is practically no effect.

We have in the tables four sets of figures that represent the effect of the filling on the estimates of *D* and of *R*. These figures are based on the average and the median of each of the two groups of experiments. I think we may safely infer that when the average and the median for any given interval of the same group have opposite signs, there is no clear effect due to the filling. The common result for these four modes of comparison would then be that the effect of the filling was to make the time seem shorter to *D* during the intervals of one, two, six and ten minutes. The two groups give conflicting results for the intervals of one half and four minutes, so that the position

of the filling — whether it came in the first or second of the intervals — was the more important factor. All four modes of comparison agree that to *R* the filled time seemed longer during all intervals except one half and one minute. In these two intervals the position of the filling is again the chief factor. In the case of the third subject, *S*, we have but one group of experiments. Here the filled time seemed longer at one minute but shorter at two, six and ten minutes. At one half and four minutes there seems to be no clear effect due to the filling.

It is evident from these results that the filling does not affect all three subjects alike. In general, the filled time seemed shorter than the empty to *D* and *S*, but longer to *R*, though there are exceptions in all three cases.

TABLE V.

SUBJECT *S.* *E–E.*

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	43.6 secs.	+ 43.6	20.4	40.5 secs.	+ 35	25.4
1 "	1 min. 6 "	+ 10	15.7	1 min. 2 "	+ 3.3	27.7
2 "	2 " 26.4 "	+ 22	27.7	2 " 47 "	+ 39.1	21.9
4 "	3 " 39.9 "	— 8.3	17.2	3 " 47 "	— 5.4	16.1
6 "	5 " 48.5 "	— 3.1	26.6	6 " 2 "	+ 3.3	24.8
10 "	10 " 10.6 "	+ 1.7	15.4	11 " 5 "	+ 10.8	5.9
	F–E.					
½ "	44.2 secs.	+ 47.3	21.3	39 secs.	+ 30	21.5
1 "	1 min. 40.9 "	+ 68.1	15.3	1 min. 44 "	+ 76.6	16
2 "	1 " 56 "	— 3.3	15.8	1 " 55 "	— 4.1	15.9
4 "	3 " 50.4 "	— 4	15.4	3 " 33.5 "	— 11.2	15
6 "	4 " 42.2 "	— 18.8	10.8	4 " 22 "	— 27.2	9.4
10 "	8 " 59.1 "	— 10.1	31.1	9 " 3.5 "	— 9.4	11.2

Difference due to Filling.

	Average.	Median.
½ min.	+ 3.7 per cent.	— 5 per cent.
1 "	+ 58.1 "	+ 73.3 "
2 "	— 25.3 "	— 43.2 "
4 "	+ 4.3 "	— 5.8 "
6 "	— 15.7 "	— 30.5 "
10 "	— 11.8 "	— 20.2 "

The estimates of empty times as compared with empty times, of the three subjects, as shown in Tables III., IV. and V., are in all respects comparable. If we consider only those intervals

where the average and the median are of the same sign, as decisive, we have for *D* no apparent error at one half or one minute, a positive error at two minutes, a negative error at four and six minutes, and no error at ten minutes.

The results do not show a constant negative error, such as was found by Michael Ejner for intervals of one half, one, two, three and four minutes marked off by sound. I found in taking the estimates that when a short interval followed a longer one it was in general lengthened. This fact may in part account for the overestimation of the shorter intervals.

When we compare the estimates of empty times of *D* and *R* with those of the first group, Tables I. and II., we find that *D* has lengthened his estimates in this second group (compare Tables I. and II.). This change was not, I think, due to practice so much as to the increased strain of attention demanded by the lack of knowledge of the probable length of the standard. *R* has decreased the estimate of one half minute, and, in general, made the estimates of Table IV. smaller than those of Table II.

For all intervals longer than one or two minutes my subjects expressed a dissatisfaction with their estimates and felt that they made little, if any, difference between the longer intervals—all times seeming very long and very much alike. *R*, at times, could not *consciously* note any difference between standards of two and six minutes, or between those of four and ten minutes, even when they followed each other in close succession, though her *results* show a constant and decided difference. *D* had a better idea; for, when asked how long he thought an interval had been, his verbal answer more nearly approximated the duration he had just marked off as 'equal' in the experiment. *S* entered the experiments with a general knowledge of the lengths of time that were to be used as standards, though ignorant during the experiments as to what particular one was being given him—but beyond two minutes could not with any constancy identify them and tell whether the standards had been four, six or ten minutes.

During the longer periods it was impossible to keep the attention so closely fixed as during the intervals of one half and

one, or at most two minutes. It is at about this point that the change of sign occurs in the estimates. The general feeling of weariness seemed to be the chief criterion in the longer intervals.

The difference due to the filling was, I think, merely a difference in the direction of attention, the monotonous regularity of the lights being, in general, a means of holding the attention and preventing the mind from wandering. From this point of view the filled time was psychologically the more empty or barren of the two—the time being filled with monotonous sensations of light, but empty of vivid or interesting trains of thought. In looking back over it, then, there would be fewer changes in consciousness to remember, and hence the time would seem shorter. This would be in keeping with the fact that the increased mental activity produced by certain narcotic drugs makes time seem long; the person, on recovery, remembering the vast number of his experiences, overestimates the time.

What is the result of these experiments as compared with Dr. Meumann's, and with the space illusions of sight and touch? We find that in sight a space is overestimated when it is filled; an interrupted line will seem longer than a continuous one, a line divided into more than two parts longer than an undivided one of the same length. In touch, while an interrupted line of 10 mm. is underestimated, yet a longer one, 10 cm., will be overestimated when it is interrupted.[1] Whether the effect of the filling in these time intervals corresponds to that in the space illusions would depend on which of the two times we consider to be the filled; for, in these long intervals the sensations of light have but an indirect influence, and are not the only filling, nor the chief factor in the appreciation of time. Taking it, however, as ordinarily understood, we do not find here a constant negative error such as Dr. Meumann found in his longest times, although to two of the subjects, *D* and *S*, the filled time in general seemed shorter.

The results of the third subject, *R*, are more in accord with the space illusion of sight, and of touch when the standard is one of 10 cm.—the filling making the stretch seem longer.

[1] See the accompanying paper by Miss Robertson, already referred to.

To test the question as to the effect of single and multiple divisions of time, and to determine whether in the temporal estimate there was anything like the space error in vision, where halving produces a negative error and more divisions a positive one, visual intervals of 3, 6, 12, 18, 30 and 60 seconds were, by the same method as that described above, divided into halves, thirds and fourths.

Under each interval ten estimates were taken for an empty time, and ten for each character of filling, on each of two subjects. A pause of two seconds was made between the standard and the compared time, the compared time being here always empty. In order that the subjects might know when the end of the standard had arrived, the word 'now' was spoken immediately after the last flash of the standard. A stop-watch measuring two tenth seconds was used by the experimenter to mark off the estimate. Although there was a reaction error here, yet it was common to both sets of experiments alike, and so might be neglected in comparing them.

TABLE VI.

SUBJECT *R.*

Interval.	No. of Lights.	Average.	M. V. %	Median.	M. V. %
3 secs.	2	3.94	10	4	9
	3	4.24	24	4	25
	4	4.71	15	4.7	14
6 "	2	6.28	16	6	16
	3	6.88	20	6.5	20
	4	8.82	16	8.4	16
	5	8.80	15	9.2	14
12 "	2	10.62	17	9.8	17
	3	10.96	13	10.4	10
	4	12.88	9	12.2	9
	5	13.38	8	13.8	7
18 "	2	13.24	16	13	15
	3	13.46	22	13.3	21
	4	16.44	13	17.3	12
	5	14.10	9	13.6	8
30 "	2	20.04	16	19.1	17
	3	20.22	23	19	20
	4	19.66	20	16.6	29
	5	24.58	26	23.9	22
60 "	2	32.84	26	28.7	26
	3	35.90	35	30.9	37
	4	39.76	19	40.1	19
	5	35.56	19	32.7	16

TABLE VII.

SUBJECT *Rd.*

Interval.	No. of Lights.	Average.	M. V. ±	Median.	M. V. ±
3 secs.	2	3.74	19	3.8	18
	3	4.56	13	4.5	13
	4	4.28	10	4.4	10
6 "	2	4.80	8	4.8	6
	3	6.68	17	6.9	16
	4	7.44	16	7.1	15
	5	8.36	12	8.7	12
12 "	2	10.07	14	10	14
	3	10.34	19	9.3	20
	4	11.11	22	9.9	22
	5	11.48	16	11.6	16
18 "	2	11.05	12	10.3	14
	3	13.83	11	14.35	9
	4	12.76	14	13.8	11
	5	15.35	12	15.3	10
30 "	2	17.41	14	16.3	14
	3	18.17	13	17.75	13
	4	17.99	18	16.35	16
	5	18.63	11	19.4	16
60 "	2	30.64	12	30.4	11
	3	34.56	16	34.3	17
	4	30.71	11	29.9	11
	5	33.54	17	34.2	16

The result we find (see Tables VI. and VII.) is that whether the average or the median be taken as the basis for comparison, the empty time seemed shorter than the filled, and, in general, the time seemed longer as the number of impressions was increased. For the three longer periods—eighteen, thirty and sixty seconds—the standard when divided into halves seemed longer than when divided into thirds to *Rd*; while to *R* the standard when divided into thirds seemed longer than when divided into fourths, during the intervals of eighteen and sixty seconds.

It is probable that in these longer periods the attention is not held closely to the sensations of light, so that other factors play a greater part in determining the estimate. In the shorter intervals, however, the mind can be kept relatively empty, so that the sensuous filling is the chief measure of duration. As long as the attention could be concentrated on the sensations, the number of lights would, I think, affect the estimate. Cases where in the present experiments the standard was subdivided

into thirds could always be consciously distinguished from those divided into halves, but many times where the standard was divided into fourths it could not be distinguished from thirds; more than fourths, I feel sure, could not have been apprehended without counting.

From these results we would say that in relatively short times as well as in spaces, the estimate is influenced by the number of impressions that fall within the stretch. There is no evidence whatever of a shortening up of the estimate due to the division of the standard into halves, such as is found in vision.

With *Rd* the division of the standard into fourths always gives a greater estimate than the division into thirds, but the estimate of thirds is often less than that of halves. *R*, with but three exceptions, increases the estimate as the number of divisions increases.

We find a great similarity in the absolute durations given in the estimate of the two subjects. Practice on longer intervals does not enable *R* to judge these shorter intervals any more accurately than *Rd*, who had no former practice, nor does it tend to reduce the variation; this being as large as in the former experiments and somewhat larger than that of *Rd*.

The position of the indifference point—where there is no absolute over- or underestimation—lies with both *R* and *Rd* between six and twelve seconds. The overestimation of three and six seconds may be due to assimilation with longer intervals. This would correspond with the results of Estel,[1] who found that an interval of three seconds, when it followed one of two seconds, seemed shorter than when it followed one of four seconds. This would also explain the shortening up of the estimates of empty intervals of thirty and sixty seconds in the case of subject *R* as compared with her former estimates given in Tables II. and IV.

As a final result of these experiments we find, in intervals of time ranging from three to sixty seconds, evidence of a temporal illusion very similar to the space illusion of sight. Both in time and in visual space, when there is more than a single division, the filled stretch is overestimated. We do not find,

[1] *Philosophische Studien*, II., p. 55.

however, that a single division shortens up the temporal estimate. This is in keeping with the space illusion of touch when the standard is 10 cm., but opposed to Dr. Meumann's results, as he finds intervals from four to nine seconds are underestimated when a single division is used.

As we increase the length of the standard interval to minutes, we do not get a direct reversal of the effect of the filling such as is found in touch; but we find the illusion either decreases or is entirely lost.

THE RELATION OF AUDITORY RHYTHM TO NERVOUS DISCHARGE.

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The term rhythm has commonly been employed in two acceptations. In its elementary signification the word describes a succession of events which follow one another with temporal uniformity; in sensory experience it is the periodic recurrence of a given stimulation; in connection with the sequences of auditory sensation it is the regular succession of sounds and silences. Such rhythms have been called *primary*. The second form, in contradistinction from the preceding, has been called *secondary rhythm*. It involves not only the regular recurrence of sensory stimuli but also a periodic differentiation among them. In this form of rhythm the unit is not a single element of sound but a group of such elements. Each of these unit groups is composed of all the sensory components lying between any one occurrence of the periodic differentiation and its proximal return.

Concerning the basis of this unification and segregation of groups theories have been advanced depending upon either of two factors, or upon their combination. It has been said that the essential principle of all rhythmical synthesis is to be sought in the temporal grouping of the sensations which support it. It has been asserted, on the contrary, that it is dependent solely upon the accentual variations whose recurrence characterizes the series. It has been urged, finally, that it is conditioned upon both of these aspects equally.[1] In the report of an experimental investigation on rhythm, published by the

[1] Theories of temporal synthesis: Hauptmann, Herbart, Lotze, Westphal. Theories of accentual synthesis: Guest, Tyrwhitt, Kostlin. Theories of interdependence: Meumann, Riemann.

writer in a monograph supplement of this periodical,[1] it was maintained that only the third of these hypotheses is consistent with the facts. No rhythm is presented by a series which perfectly fulfils the temporal conditions, so long as no recurrent accentual differentiation arises; and such recurrences of accent, on the other hand, are equally unable to support the impression of rhythm unless they fulfil specific temporal conditions in the form of their succession.

The elementary condition of the rhythm phenomenon is the periodic accentuation of an auditory succession occurring under specific temporal relations. While accentuation is essential to the appearance of rhythm, no specific mechanism is necessarily involved in its production. If forms of objective differentiation appear in the material, the variations which support the impression of rhythm may be indifferently of the intensity, quality or duration of the stimuli. Rhythm, therefore, does not consist of the presentation in consciousness of a regularly recurrent intensification of certain elements in a succession of auditory stimulations, for the impression is as distinct and adequate when the only differentiation presented by the series consists of a periodic lengthening of the component sounds or intervals. If the nature of the experience as a modification of consciousness be the same in these various cases, the essential constituents of the impression must be looked for elsewhere than in the special nature of the qualitative types of variation embodied in the sensory series, especially when there is taken into account also the fact that this impression is aroused when all such objective differences are absent, and the sole condition is observed that the stimuli follow one another at a certain rate.

The impression of rhythm depends, secondly, upon the repetition of the periodic differentiation manifested by the sensory series. That impression never arises from the presentation of a single differentiation period; it does appear at once and perfectly with its first recurrence. In other words, a simple measure cannot constitute a rhythmical sequence; two

[1] 'The Constitution of Simple Rhythm Forms.' Harvard Psychological Studies, Vol. I (Monograph Supplement 17); now in press.

such measures do so, and two measures form the simplest conceivable rhythm. In every concrete impression of rhythm two factors are involved: Firstly, a formal element which consists in an ideal scheme of relations which the sequence fulfils, and which contributes to the series of impressions a *Gestaltsqualität;* and, secondly, a material or time-filling element which consists in the repetition of functionally equivalent simple groups which constitute the structural elements of that ideal sequence. Recurrence is essential to the movement of the rhythm only, not to its form. The latter is apprehended immediately and completely upon a single recurrence of the rhythm group or period. As the rhythmical sequence advances the apprehension of form can be affected only by complication, that is, by the rise of new rhythmic forms into which the originally apprehended simple group enters as an elementary constituent. At each step the whole character of the rhythm is given up to that grade of synthesis which the total series presents. The simple measure, the verse, the stanza, and those poems which, like the sonnet, possess a distinct figuration are severally independent rhythmical forms, each of which is apprehended through a single presentation of its structure.

Such forms are not in themselves rhythms. The impression of rhythm arises only when these formal relations are embodied in a concrete movement. Formally adequate successions are constantly presented in our experience without arousing any impression of rhythm. Under certain circumstances the single occurrence of a rhythmically related group of sounds may give rise to the impression of rhythm, namely, when the melody which contains it is familiar, or the mind is pervaded by a tendency to rhythmical expression of this particular form. In such cases, however, the simple measure does not contain the rhythm, but only initiates a process the continuance of which supports the experience.

In the third place, the impression of rhythm arises only under the maintenance of specific temporal conditions in the succession of sensory stimuli which support it. Though the superior and inferior limits of the absolute rates of succession which condition the feeling of rhythm in a series of sounds

have been variously reported by different observers, these divergences presuppose in all cases the indubitable existence of such limits. Intensive subordination among the elements of the objective series and periodicity in their recurrence are by themselves wholly insufficient to give rise to the feeling of rhythm. Were the impression dependent solely upon a perception of the maintenance of such relations in a series of auditory stimulations, the temporal limits to the experience which are actually encountered would be inconceivable. We do thus conceptually speak of geological rhythms, but this is a figurative projection of the term into realms which our experience can never penetrate. One may conceive the existence of a consciousness upon which such a series of stimulations would produce an impression of rhythm, but for the human consciousness it is forever unrhythmical, and no amount of effort at conceiving it in this way will avail to give one the peculiar experience of 'feeling a rhythm' in it. That experience is as new and different from the apprehension of the isolated elements of a slower series as the continuous musical tone produced by a siren is different from the succession of puffs which one hears when the mechanism revolves at a slower rate.

The facts involved in the elementary conditions of the rhythm impression which have important theoretic bearing may then be stated shortly as follows: First, the process of accentuation is not necessarily connected with any specific type of objective change and may arise in their absence. It must therefore be an activity which these objective factors occasion only and do not contain, and its appearance is most closely connected with the temporal relations embodied in the series. Second, the scheme of a rhythm group, in its temporal and intensive relations, gives only the formal conditions for the appearance of the rhythm impression. For its realization the accentuation of certain elements of the series must be periodically renewed, and this repetition of functionally integrated groups persists through all grades of structural synthesis. Third, of all the possible rates of succession which a series of auditory sensations may present the feeling of rhythm accompanies those only which fall within a narrowly limited range.

Above and below these limits the succession of stimuli fails to support the experience. It cannot there be mediated either through the intensification of objective accentual differences, nor by reinforcement through any type of subjective emphasis.

Upon the intimate nature of the process of rhythmical accentuation in its relation to the phenomena of temporal limits must turn our theories as to the nature and fundamental conditions of the rhythm experience. In the analysis of this process it is important to discriminate purely rhythmic factors from concomitant but independent elements of experience. To the latter class belong all complications and enrichment of the bare limiting stimuli upon which the definition of periodicity depends. It is necessary, above all, to inquire by what means the rhythm experience is superinduced upon the presentation of stimuli which do not manifest those differences with which its arousal is ordinarily connected. The existence of such contributed or subjective forms makes it necessary to take into account the possibility of its presence as a transforming agency in every experience of rhythm, objective as well as subjective. If it create rhythm where none previously existed, it may complicate and enrich simple rhythm forms produced by the voice or instrument which affords the sensory material of experience. In point of fact this subjective contribution is not restricted to series from which intensive and temporal differences have been eliminated. It appears equally in the organization of recurrent forms which possess a distinct objective configuration. Under specific conditions of temporal succession such identically repeated forms — for example, successions of rapidly uttered dactyls — are subjectively differentiated into alternate groups having the relation of major and minor phases, in which both temporal and intensive values are quantitatively unlike for the apprehension of the hearer. Upon this constant subtle accompaniment the characteristic form and affective overtone of the rhythm impression in some degree depend, and it is at least conceivable that the essential element of the experience should lie in the subjective contribution itself—that rhythm is never a fact of perception alone, but essentially involves an active attitude on the part of the apperceiving subject. This is the conception to

which the writer was led in the course of the investigation already referred to, and the point of view may be stated in a few words. *Rhythm is always produced.*. The bare auditory perception of a series of sounds, the uniformity of which is broken by periodic reinforcements, no more affords the peculiar experience of rhythm than does the perception of those visual symbols which represent the relations of such a series of sounds in musical notation. The successive stimulations must start a series of motor impulses somewhere before its rhythm is felt. Apart from such a pulse of bodily change the perception of a rhythmical series of sounds would be the bare abstract apprehension of their varying intensities and intervals.

The fundamental conditions of the rhythm experience are therefore to be looked for in the laws of periodicity of functioning in the bodily organism. It is because these processes take place under conditions of regularly recurrent change that the time element becomes important in rhythmical sequences. The existence of beautifully proportioned durations in rhythm is a purely derivative fact. We do not take pleasure in such series because of the proportion or simplicity of their relations, but because their patterns are reproductions of the natural forms of our own activities. The pleasure which rhythm affords arises from the coincidence of subjective and objective change.

In regard to the nature of the mechanism involved in this process of subjective activity, with the phases of which the elements of objective stimulation must be specifically coördinated in order that the impression of rhythm shall arise, we may conceive, in the first place, a periodical facilitation and inhibition of nervous activity to arise from the relation between the periodicity of its own rhythm of functioning and certain intervals in the objective series of stimulations. If such a physiological rhythm appears in the functioning of the central nervous system, a periodic increase and decrease should occur in the intensity of the sensations coördinated with a series of unchanging stimulations, according as its elements were correlated with positive or negative phases of the nervous activity. The effect would be analogous to that consequent upon successive tensions and relaxations in the drum of the ear, and the rhythm must be

called, in so far as the sensory experience is concerned, an objective rhythm, illusion arising only in connection with the interpretation of the source of the positive changes which appear in consciousness. In the second place, we may conceive the succession of auditory stimulations to arouse a parallel motor accompaniment in the form of sensation reflexes occurring in some part of the bodily organism. The impression of rhythm under this conception is due to a system of kinæsthetic sensations whereby periodic elements of the primary auditory series are reinforced in such a way that the whole sum of sensational material rhythmically increases and decreases. This active accompaniment may be conceived to take the form either of an accentuation of certain members of the series only, or of a continuous accompaniment in which a reaction is coördinated with every element of sensation, the violence of the motor discharge periodically increasing at those points in the series which form the successive accents. Here, also, there is present in consciousness a real rhythmical series, but it is the accompaniment, not the original sequence of sensations, which is thus characterized. Both of these relations between the rhythmically repeated stimulation and the nervous activity, namely, functional facilitation and reflex motor discharge, I conceive to be represented in the conditions which support the impression of rhythm. The sole existence of the former type of effect is the theoretic limit of a process which in its actual occurrence always involves elements of the latter kind.

The components of this rhythmical accompaniment present wide variations. The muscular contractions which mediate it are to be looked for among the most mobile members of the body. The tongue, the head, the jaws, the fingers and the feet are visibly employed in keeping time to rhythmical stimuli. Of greater prevalence but much more difficult of observation are contractions giving rise to sensations of strain in the throat, head, chest and limbs, tensions in the vocal and respiratory muscles, and above all the simultaneous innervation of opposed sets of extensor and flexor muscles producing alternate phases of rigidity and relaxation which do not affect the local relations of the organ in which they take place. In the apprehension of

rhythm forms this kinæsthetic comment appears ineradicable. For each individual the inhibition of the natural set of motor accompaniments interferes with the impression of the rhythm. In many cases when these expressive movements are eliminated the sense of rhythm is lost. The process of voluntary inhibition of particular modes of apprehending rhythmical material consists in breaking up the special set of motor emphases which supports that form of perception. The endeavor to apprehend the sensory sequence under a different rhythm form is uniformly accompanied by the motor emphasis of those accentual points which are characteristic of the novel rhythm. In the attempt to perceive as monotonous an undifferentiated series of sounds which has hitherto supported a subjective rhythm the recurrent motor accentuations of the previously dominant rhythm are got rid of, not by a suppression of all accompaniment, but by the equal emphasis of each element as it appears. Finally, the voluntary inhibition of such sensation reflexes is a process which affects only a limited number of factors; it cannot be carried out in any completeness. Sufficient rhythmical accompaniment may in all cases escape notice to give definiteness and character to the rhythm.

This process of secondary motor response is not necessarily confined to the voluntary muscle groups. It may be reflected into the mechanisms of accommodation in the organs of perception through the periodical renewal of sensorial attention. The effort to attend to a sensory stimulus involves, as part of the concomitant organic adjustment, an adaptive change in the condition of the sensory apparatus which increases the sensitiveness of its response to incoming stimuli. Every return of the attitude of attention to auditory stimulation temporarily sharpens the sense of hearing by drawing to a nicer tension the membranes of the drum of the ear. In such a case we have to do with a process which is not itself the object of voluntary adjustment but the secondary result of such an activity. For periodicity in the recurrence of such an attitude we may look to either of the two sources already stated, a physiological rhythm in the nervous mechanism of sense perception itself, whereby the tonicity of the organ suffers regularly recurrent changes, or an

adaptive process concomitant with the rhythmical motor accompaniment of the sensory series, those elements which are emphasized being likewise more closely attended to, and thereby receiving reinforcement as sensation intensities. The pulses of attention in the apprehension of rhythm are at least not more rapid than the recurrence of the simple group, for such structural units are without exception apperceived as unities and not as a succession of separable parts.

In the rhythmization of undifferentiated auditory material the relation of the processes of sensory accommodation and motor innervation to the stimulation series becomes of paramount importance. In it, while the rhythmical form is a purely subjective contribution, the illusion of objective differentiation is complete. Subjective rhythm appears only under temporal relations of narrower range than those which condition objective forms, but within these limits it arises spontaneously. It is not, however, strictly or uniformly uncontrollable; the establishment of such a form of apprehension does not necessarily involve its perpetuation during the continuance of the auditory series. In certain cases the mere attending to the apparent rhythm causes its disappearance. When of a more integrated type the rhythm is commonly suppressed if each element be regarded in isolation from the succession of which it forms a part. When attention drifts away and the series of sounds — or as much as can be grasped in a single act of attention — is regarded as a whole, the rhythmic differentiation reappears. In more obstinate cases the method of breaking up the rhythm is to emphasize, by a more or less violent reaction, those elements which fall into unaccented positions in the series or to emphasize equally each sound as it appears. The rhythmic apprehension of undifferentiated material is also subject to frequent fluctuations both in its form and continuity. It appears, undergoes change of structure, is dissolved and reappears within relatively short periods of time. When the experience is supported by the conception of an ideal form which the series of stimuli fulfills, the apprehension of the material in a single mode may be indefinitely prolonged. In the experience of rhythm, then, whether supported by a succession of sensa-

tions presenting figured groups, or by a uniform series, there is presented a process of apprehension penetrated at every point by secondary motor impulses.

It will help us to understand the way in which this motor accompaniment is aroused in the presence of a rhythmical series of sounds if we recall the primitive relations of reaction to stimulation. In concise form it may thus be stated: Every presentation tends to arouse some movement. In kind this movement is imitative of the original. A succession of regularly recurrent stimuli, therefore, tends to set up a process of rhythmical movement. Not to accompany the presentation of a stimulus in this way indicates inhibition of some sort.

But this is not an explanation of the tendency to prefer one type of movement to another, to imitate a rhythmical but not an unrhythmical series of stimulations. The unrhythmical succession is vastly more frequent in our experience than the rhythmical. If the establishment of particular types of organized movement depended upon the frequency with which the like relations were presented in the world of our objective experience, we should prefer forms of irregular movement. The actual tendency, on the contrary, is toward the embodiment of rhythmical relations in our movements; we maintain a tempo, we beat time, we accompany a rhythmical series of sounds but not an irregularly recurring series. The relation to experience is not that of the establishment of a simple correspondence of subjective habit with objective conditions.

There is present in such processes a factor which is reinforced by stimuli occurring in periodic succession but which is unsupported, or inhibited, by unrhythmical series. This is the law of nervous action in virtue of which the form of a movement once originated tends to be perpetuated. The kinæsthetic sensation aroused by the perception-reflex, or imitative reaction, is itself a presentation having the nature of an incitement to the repetition of the movement. It partakes of the character of the original stimulus which provoked the primary reaction, and tends to bring about again a discharge into the same complex of muscles. Thus, in a nervous system uncomplicated by other simultaneously active processes, the origina-

tion of any movement tends toward the establishment of a rhythmical series of reactions by the reciprocal arousal of movement and kinæsthetic sensation within a single closed arc.

This primitive condition of activity is disturbed, in the organism possessed of a highly developed nervous system, both through the interference of intense outward stimulations occurring at irregular intervals, and by counter-suggestions to action of a conflicting type where ideal associations are present. Even in such organisms when these factors of complication are withdrawn the process may spin itself out indefinitely. This dissociation of the motor process from the control of ideal associations takes place under both normal and pathological conditions. In the normal subject it is presented in states of inattention to those minor tensions and reactions of the body which are either mechanized or unconnected with the purposeful activity of the moment. Such are the rhythmical lilting, thrumming and beating of time into which the mobile fingers and tongue fall during moods of idleness or abstraction, and the larger innervation processes of the body which have grown thoroughly habitual, such as the series of reactions by which the process of walking is carried on. The same sequence of relations is presented, in connection with a simpler type of associative system, in the habit of young children endlessly to repeat single sounds or imitative movements.

The pathological forms of this process are many. It appears during the transient suspensions of rational control occurring in fever delirium, in the meaningless repetition by the patient of a word or syllable which he has uttered or heard. It is presented in the tetanic innervation of the muscular system characteristic of catalepsy, and is probably represented also in the continued reproduction of a suggested movement by the hypnotic subject until the process is arrested by the hypnotizer. It is manifested in more pronounced and obstinate forms among idiots and the insane, where its exaggerated and persistent types have led to the coining of the descriptive term 'echolalia.' I cannot agree with Professor Ziehen's extension of a pleasure valence to the origin of these reactions, though accepting his first statement without reserve when he says: "As regards the

succession of sensations, therefore, a regular periodicity is the chief condition for the appearance of feelings of pleasure. It is not mere chance that maniacs and those afflicted with emotional paranoia often speak in rhythm and rhyme. Such phenomena harmonize rather with the morbid, positive emotional states characterizing these forms of psychosis."[1] These are forms of activity to be explained not by the concept of a deliberate pleasure-seeking but as the expression of a primitive, uncontrollable impulse to utterance, a reversion to a simpler type of activity, in which the elementary rhythms of motor innervation are uncomplicated by a richly developed system of associated ideas.[2]

The general conditions underlying all these phenomena are characterized by simplicity or primitiveness in comparison with the workings of the normal mature nervous system — conditions in which the sensori-motor arc manifesting activity is in relative functional isolation, such that the interferences with its characteristic forms of innervation by other simultaneously active brain processes are reduced to a minimum. Primitively it needs not a regular succession of sound stimulations, an objective rhythm, to establish a rhythmical series of movements; a single originating stimulus, if it be sufficient to bring about the initial reaction, will thus serve to set up a succession of repetitions. The fortune of this series of reverberations does not depend solely upon the type of functioning represented by the nervous arc within which the reciprocal movements are conceived to take place. It is subject to influences both from beyond the organism and from within it, either of which may tend toward reinforcement, on the one hand, or inhibition, on the other; and upon the relation of the sum of these extraneous factors to the arc primarily excited the subsequent course of the series of reactions depends.

[1] Ziehen: 'Physiological Psychology,' p. 148.

[2] This is characteristic also of the spontaneous rhythmization of rapidly succeeding homogeneous sounds, of which one observer in the experiments already referred to writes: "I certainly never try to do it; and, so far as I can see, it is never prompted by any desire to get satisfaction. It seems to be a spontaneous act, which when it arises causes satisfaction in a very slight degree."

Reinforcement of the primitive tendency to repetition may come either from a regularly repeated external stimulation,[1] or from the idea of a series of rhythmical movements to be carried out. The process set up by a single stimulation is unlikely, under any actual set of conditions, to establish successfully a system of rhythmical movements. The probabilities are immeasurably in favor of its being lost in the complicated set of independent sensational and ideational activities with which the general system is throbbing. When, however, the original stimulation is regularly renewed, the waning process receives periodical reinforcement by positive increments of stimulation and the reactive motor accompaniment waxes from moment to moment until a fully developed set of rhythmical movements is established. Under this conception the character of the motor accompaniment, as also the intensity of the rhythm feeling, should undergo a certain growth. The former should not appear either so vigorous or so exactly coördinated in the first phases of response to the recurrent stimulation as in its later forms. We should expect it to be susceptible also of indefinite exaggeration until conceivably the diffusion of the wave of motor response involved the whole body in violent changes. As a matter of fact we know from common observation that the tendency to accompany an auditory rhythm by movements of the bodily members, by humming, lilting and singing in tune, by beating time with finger, or foot, or head, by waving the arms, swaying the body, etc., does not arise at once upon the initial stimulation, nor is it at first fully developed. It manifests a period of latency and a process of growth, requiring an appreciable time to attain its maximum. One can observe this

[1] Within the category of such external stimulations we must of course include the secondary sensations, of sight and sound, which accompany the kinæsthetic feelings as common products of the movements involved in beating out a rhythm. The exactness and perpetuation of the rhythm habit depend upon the nature of these derivatives of the motor reaction, as well as upon the resident sensations themselves. In proportion as these limiting stimuli become distinct and forcible does their value as incentives to the repetition of the reaction increase. Sensations of movement, kinæsthetic and visual, of resistance, and of sound all combine to reinforce the intensity of the rhythm activity; and it is probable that the most effective union of resident with secondary sensations is attained in the process of vocal utterance.

reinforcement in the manner in which an audience accompanies a piece of music by gradually increasing movement until the whole roomful of persons is swaying and beating to the rhythm like a single instrument.

One may mark its persistence, too, in the difficulty met with in the attempt to break up such a system of rhythmical movements when once thoroughly set agoing. The obstinate running through the head, even to a pitch of distress, of a tune which one has heard, the habits of strumming and lilting which establish themselves as the reflex accompaniments of the return of certain attitudes, and which cannot be shaken off, are examples of this tendency. We should include within the same category the fact that the subjective rhythm which appears when uniform sounds follow one another at certain intervals of time does not manifest itself at once on the inception of the series, as is the case with the earliest measures of an objective rhythm, but develops slowly, the appearance of this contributed rhythm being dependent, as has been already said, upon the development of a rhythmical process of discharge within the motor centers. As the concomitant of such a developing system of reactions within the organism we should look for an increase in the affective over-tone, and this mounting rhythm feeling, with its undeniable pleasurable quality, is doubtless itself a positive factor in accentuating and perpetuating the process.

We should also be prepared to find a development of sensitiveness to rhythmical relations, manifested in a progress toward uniformity among the successive reactions and a refinement of perception in regard to irregularities occurring within the series of stimulations. Both of these forms of change appear in the results of the experimental investigation already referred to. On the one hand, both unit measures and larger series of reactions show a progressive integration, manifested in the reduction of the indices of mean variation, and on the other, the threshold of just discernible variation from type of structure is similarly lowered as the series of rhythmical sounds advances toward its close.

It is obvious that in the setting up of such an organic rhythmical accompaniment the temporal relation of the series of stimu-

lations to the nervous habit of the organism must be a factor of great importance. If there is to be objective reinforcement of an organic tendency to reproduce the movement, the phases of the two processes must coincide, or at least approach within the limits of adjustment on the part of the organism. The elements of a regularly recurrent stimulation may be conceived to exert an inhibitive influence equally well with any incidental or irregular form of excitation, if its periodic phases are in opposition to those of the natural rhythm of the sensori-motor process itself. Similarly, it will altogether fail to support the tendency if its recurrences are so infrequent that the impulse to reaction has died out before the following stimulation takes place. What particular intervals between successive stimuli are most favorable to the establishment of the process is a matter to be determined empirically by direct experimentation; but apart from the actual determination of the values of such limits we should expect that the tendency to rhythmize indifferent sensory stimuli, or to accompany them by rhythmical movements, will depend upon the relation of their absolute rate of succession to the natural rhythm of the bodily processes involved.

That such limits exist has long been established; it requires but slight observation to discover them. Below a certain rate they fail to arouse any involuntary response; between this and a superior limit the tendency to segregate and accentuate the material and to accompany it by motor reactions is practically uncontrollable; beyond this upper threshold the organic adjustments fail to follow the accelerated series, though here, under certain conditions, a new form of rhythmization may supplant the old, the impulses proceeding not from the single elements of stimulation serially but from the integration of successive groups which now replace those elements as units.

The rhythm activity, then, represents a relatively undifferentiated type of reaction. Its appearance as a spontaneous exercise and as a reflex accompaniment is a manifestation of the primitive tendency to reaction toward presented objects, and of an equally primitive tendency to perpetuate a movement once made. It belongs to a class of activities which we habitually connect with early ages of development and with the lower parts of the

nervous system. The opportunity for the outcropping of these primitive activities is presented under conditions in which the higher brain processes are inhibited, or reduced below their normal preponderance. In such temporary moods of the normal subject as revery and abstraction, in the lack of occupation and of mental strenuousness, we should look for conditions favorable to the development of spontaneous or reflex rhythm activities. Now it is just in such moments of relaxation and mental drifting, when consciousness is occupied with no serious or pressing duty, that the impulsive rhythm habit, humming, tapping, singing, and the like, and the reflex habit of accompanying any objective series of regularly recurrent stimulations, characteristically crop out. The active association of ideas is either suppressed or drafted off into an entirely different set of centers than those concerned in the production of the rhythmical reactions. We are either mentally vacant at the moment, and at the mercy of vagrant stimuli, or we are absorbed in some mental process which makes no tax on the periphery of the body, and are usually unaware at the moment of the rhythmical reaction process which, thus accidentally originated, is spinning itself out in the absence of inhibition from the higher centers of the brain.

Further, the persistent and exaggerated types of rhythmical motor activity presented in the abnormal conditions already referred to confirm this view of the relation of the rhythm habit to the general activity of consciousness. In fever delirium the processes of orderly association characteristic of normal life, by which random meaningless movements are inhibited, is interrupted, the primary reaction centers of the lower brain are released from the inhibitive control of the cortex, and the utterance or other movement, once originated, tends to a prolonged repetition, limited only by the final exhaustion of the muscular or nervous system. Similar conditions are presented in the tendency of children and idiots to prolong rhythmical repetitions of movements and to rhyme over sounds to themselves. The prevalence of the element of rhythm in primitive music and ceremonial is indicative of a lower stage of development than our civilized type. An indispensable condition of the

purity of the rhythm activity is its independence of a changing ideational content. The most perfect rhythm, whether simple repetition of a motor reaction at regular intervals, or the complex grouping of such rhythmic material into synthetic units, is that which is most free from secondary significance. Its movement is inevitably clogged when it is made a form in which to cast discursive thought. The most adequate rhythms are those which present only a pure sensory stimulus to the motor process, uncharged with symbolic value, which allow the attention to center in the reciprocal play within the sensori-motor arc, and make no draft upon it by a stream of changing ideas which the limiting stimuli are made to suggest. Its purest and most effective embodiment is found in such sequences as that of the drum-tap, the most potent incitement to the rhythm habit of all forms of auditory stimulation which have been invented. Add any new element to this perfect instrument and its effectiveness in arousing the feeling and evoking the accompaniment of rhythm is proportionally weakened. Introduce change of pitch and give melody to the series of sounds; combine the beating of the skin with the tones of vibrating strings, or pulsating air, or quivering reeds, in chords, harmonies and orchestral effects; above all, make the series of sounds which sustain the rhythm the vehicle for a train of mental images of objects and relations, with their ever-changing influence upon the direction of the attention process, and the rhythm is thereby impaired, proportionally to the capacity of the factors of melody, harmony, or rational significance to attract attention to themselves and away from the pure rhythmical element. "Almost every civilized country has its national melodies, in the form of songs, dances, or marches. While most of these melodies have remained local, some of them, particularly dances, have been taken up by the great masters and introduced into general musical literature; and though many of the original melodies have long been forgotten and changed, yet the form and characteristic elements of them have remained.

"The great masters who employed certain national dance-forms for special compositions, or introduced them in their greater works, although describing the general distinctions of

these dances, did not adhere strictly enough to the more detailed characteristics; and handled the form with such individual freedom that it became under their hands an artistic dance-form, but ceased to be a dance in the popular acceptation.

"Thus, the dances of Bach; the minuets of Hayden, Mozart, and Beethoven; the waltzes and polonaises of Schubert; the mazurkas, waltzes, and polonaises of Chopin, etc., are artistic forms; and not intended for practical dance purposes."[1]

The transposition and complication of accents which these art-forms involve disguise their primary rhythms, and make difficult or impossible the coördination with them of the rhythmic phases of movement upon which the dance depends. Dominant and effective rhythm can exist only in simple musical and poetical compositions. It is not in these compositions that the most highly differentiated and complexly organized types of rhythm are to be found; but in them the rhythm factor is greater relatively to the whole production. The person in whom discrimination of pitch-differences and melodic or harmonic relations is poor, and for whom the coördination of groups of tones is difficult, finds his enjoyment of musical performances in the pleasure of the immediate rhythm experience. When this is overlaid by those more complicated types of co-ordination which delight the trained musician's ear, his pleasure vanishes, for the composition then produces no distinct effect upon him, it gives no incentive to rhythmical accompaniment and appears only as a confused medley of sound. One gifted with a musical ear or trained in the appreciation of musical effects, who follows and enjoys such a composition, does not find his pleasure simply in a rhythm which is of a more synthetic type than his untrained neighbor is able to grasp. His attention is primarily attracted to other elements, to the sweep of melodic phrasing, the antiphony of passage to passage, and the larger unities of composition, which a rhythmical undertone of recurrent accentuation and analogy of form enables him conceptually to grasp. He does not feel the rhythm primitively and immediately as the other does that of his tattoo upon the table or his meaningless lilting; it does not provoke in him any dis-

[1] Christiani, A. F.: 'The Principles of Expression,' etc., pp. 93-95.

cernible type of motor response; it has lost its intrinsic significance for the æsthetic subject in whose consciousness it appears, and serves chiefly as a secondary means of extending the scope of the attention process in its effort to grasp an ever wider set of pleasurable stimuli.

Still more strikingly, when the rhythmical sounds are clothed with ideas, and the sensations which support the rhythm are at the same time the symbols of rational speech, does the association of these secondary factors with the fundamental element of the rhythm phenomenon interfere with the purity and power of its effect. Poetry, like the opera, is an irrational combination of two processes, either of which, for its free activity, presupposes the absence of the other. Rational thought, fit sequence of ideas, demands a free movement among complicated elements in which no precise periodic return to the same origin is conceivable; while just this condition of a never-ceasing repetition of similar forms of relationship is fundamental in every rhythm process. For the rhythm experience is one and the same in music and in verse, the vehicle of its expression alone differing in the two cases. The proportionate relations of rhythm are more exactly fulfilled in musical performances than in poetical recitation; for the material of music, as a set of symbols, is incomparably less definite than that of verse. The draft upon the attention in the former case leads constantly back to the immediate relations of quality, intensity and duration existing in the sensuous material itself. There is no parallel series of logically connected images, as in the case of poetry, to distract the mind from the changes in the sensory material by which they have been suggested. And the conditions of musical expression make it imperative to observe the laws of rhythm, since upon these depend in great measure the processes of comparison and coördination which are superimposed upon the immediate enjoyment of the sensory qualities which the music presents.

In poetic expression the function of rhythm is essentially more subordinate, then, than in that of music. The rhythm of poetry pulls in one direction; the thought it presents, in another. The most perfect of rhythmic verse-forms is found in the recita-

tion of nursery rhymes and nonsense jingles, where the reciter is least seduced from the production of a beautifully proportioned series of recurrent groups of motor reactions by the associations of vitally significant words. The high relative value of the rhythmical element in these compositions is not due to any unusual consonance between the orthographical and the rhythmical accents. On the contrary, as Sanford has pointed out, no other popular form of poetic writing makes such audacious demands upon the rhythm sense of its reader. Rests, elisions, prolongations, even transpositions of orthographical accent are freely made, yet the ordinary reader is carried securely through their complex changes. This is so because the maintenance of the formal key-note given by the verse structure and initial orthographic accents is rendered easy by the simplicity of their content as compared with other forms of poetical writing.

In proportion as the structure of the verse-form grows more complex, or the burden of thought more exacting in the demands which it makes upon the attention, this sense of rhythm fades. Only in those pieces in which there is a return of the thought upon itself periodically in the form of a refrain, or the meaningless repetitions (meaningless from the point of view of associative thought, but most significant in their relation to the function of rhythm) of folk-songs and nursery rhymes, can the words of the verse be said to reinforce the rhythm in any way — that is, only when, at the sacrifice of their intrinsic significance, the words are used as practically pure sensory rhythm elements. It is because of the disjunction between sounds as the material of rhythm and sounds as the symbols of rational speech, and their more specific correlation in the former case with a parallel set of motor innervations, that one has a more distinct feeling of rhythm in the spoken verse of a tongue with which one is unfamiliar than in that of one's own speech; that one best clarifies the rhythmic structure of a stanza by substituting for the significant words of the poet a lilting utterance of meaningless syllables; and that the 'music' of a poem may persistently haunt one, while the words and even the thought and general topic with which it is concerned are wholly irrecoverable.

In the relation of the successive sounds of a rhythmical sequence to a coördinated system of motor impulses is finally to be sought an understanding of the constitution of objective rhythm forms and the laws of their synthesis. These phenomena include the characteristic distribution of intensities within the unit group, the apparent accentuation of an element which arises from the simple extension of the interval following it, the exaggeration of the duration of the accented element and its following interval, and the concomitant variation of intervals in dependence upon the intensive magnitude of preceding elements. In connection with uttered rhythms our knowledge of the laws under which nervous discharges take place — such as the relation of intensity in the innervation to the temporal curves of latent period, contraction and release — makes possible the prediction of certain relations of stress and duration among the elements of the group. Under this concept the alternating phases of expectation and its fulfilment which characterize the experience of rhythm will be resolved into psychological attitudes which are physiologically conditioned by systems of strains and releases due to the rhythmical opposition of those phases of motor innervation which the reaction accompaniment involves. Not only in connection with uttered rhythms, but also in regard to the forms in which objective series are apprehended, this concept should find application; for in the latter case the same characteristic relations appear between stress and duration as in the former. These connections cannot be due to differences in the course of the sensory after-images resulting from intensively varying stimuli, for the causal relations between the two may be inverted, that sound which is prolonged or followed by a prolonged interval receiving apparent accentuation. Their conditions are to be sought rather in the system of motor discharges which the series of stimuli arouses, especially in its relation to the rhythmical renewal of sensorial attention and the temporal curve of the nervous changes involved in the mechanism of accommodation.

DISCUSSION AND REPORTS.

THE RELATIONS OF FEELING AND ATTENTION.

In Professor Pillsbury's review of Zoneff and Meumann's article 'Ueber Begleiterscheinungen psychischer Vorgänge in Athem und Puls, I., (PSYCH. REV., IX., 405) occurs the following paragraph:

> **Another statement of the text-books, founded upon self-observation, which was investigated was the effect of directing the attention to the feeling process itself. The usual statement that attention to the feeling destroys the feeling was not found to be true of the effect upon the physiological concomitants. On the contrary, the effects upon both respiration and pulse are increased. The authors however state that they think the result would have been different if psychological introspection had been attempted instead of mere attention to the feeling.**

These sentences seem to me to be misleading, both as regards the text-book statements and as regards the new results. Since the point involved is systematically important, I venture to offer the following criticisms.

1. Professor Pillsbury draws a distinction between text-book statements, based on self-observation, and the experimental results of Zoneff and Meumann. It need hardly be said that the latter are also based upon self-observations. "Der Reagent wurde angewiesen, sich einmal auf den Reiz, * * * ein anderes Mal auf das Gefühl zu concentriren" (*Phil. Stud.*, XVIII., 67). The conclusions drawn by the two authors are avowedly gathered from the introspective reports of the observers. The reviewer's meaning must, therefore, be that the text-book statements are based upon casual introspections, made without experimental control. I have been guilty of 'the usual statement' in two text-books, and may be allowed, perhaps, to fit the cap to my own head. If I do this, I must emphatically repudiate Professor Pillsbury's suggestion. I have made a large number of experiments myself, and have taken introspective records from many other observers under experimental conditions. As long ago as 1894 I published a brief summary of experiments upon the question (*Phil. Rev.*, III., 431; the systematic setting of the paper is crude, but the observations are reliable). And I have no doubt that the other writers whom the reviewer has in mind also wrote from first-hand evidence.

2. It is, however, more important to look at the new experimental evidence. Zoneff and Meumann found that their results showed discrepancies, which they explain as follows. "Concentration of the attention upon a mental process" may mean two different things. It may mean, in the first place, that "one keeps the feeling, by voluntary effort, for a relatively long time at the conscious point of regard, and analyzes it. One reflects, *e. g.*, whether the feeling is more or less pleasant (or unpleasant). In this case there is a real analysis of the feeling, accompanied by a certain bodily tension. With *this* sort of self-observation of mental states, the changes of breathing and pulse must, very evidently, be those of attention." It may mean, secondly, that "the observer tries to bring the feeling as well as may be to *consciousness, without however analyzing it.* In other words, the feeling makes its way to the conscious point of regard, *but that is all; nothing more is done with it.* The feeling is, so to say, felt resignedly" (*loc. cit.*, 73: italics in the original). Both possibilities were realized in the experiments: "diese beiden Möglichkeiten kommen bei unseren Versuchen vor und deshalb diese scheinbare Resultatlosigkeit in der Tabelle" (*ibid.*). There is no word of the authors' thinking that "the result would have been different if psychological introspection had been attempted." The results in the two cases *were* different.

Zoneff and Meumann conclude that "a mere direction of attention upon the feeling intensifies it; if, on the other hand, the feeling is made the object of a psychological analysis and in this sense the object of attention, it is considerably weakened, indeed, even entirely destroyed" (*ibid.*). The first clause summarizes the facts (*a*) that certain observers, when called upon to 'attend to the feeling,' interpreted the requirement to mean, 'Let the feeling have its own way with you,' 'Resign yourself passively to it,' and (*b*) that, under these conditions, the feeling attained its fullest intensity. The second clause repeats 'the usual statement' of the text-books. But there is, surely, no contradiction. Nor is there, in the first clause, anything new. The rules for affective work given in my Experimental Psychology, I., i., 98 f.; I., ii., 151 f., 166, 181 f., tally exactly with Zoneff and Meumann's results.

The only possible point of dispute, therefore, between the two investigators and the text-books would seem to be a question of terms. When you tell an observer to attend to his feeling, and he resigns himself passively and absorbedly to it, can his mental state properly be described as a 'blosse Richtung der Aufmerksamkeit auf das

Gefühl'? One may say, I think, that his interpretation of the order is perfectly natural and justifiable. He instinctively takes up that mental attitude which favors the feeling at the expense of the other contents of consciousness. One must also say, however, that the interpretation is naïve. With more introspective experience, he gives a narrower meaning to his instructions; he purposely abstracts from sensible processes, and tries actively to attend to the affective contents as such. "It may be remarked further," say Zoneff and Meumann, "that the better an observer is practised in self-observation the more does he incline to analyze the feeling, and not to enjoy it or thrust it from him" (*ibid.*). Now Zoneff and Meumann are themselves taking a non-committal position as regards systematic questions (*cf.* Meumann's Remarks on Terminology, *loc. cit.*, 2). They may, therefore, for the time being, accept the naïve observer's interpretation at its face value, and classify his reactions as instances of 'simple attention to feeling.' But a psychology must go farther. How does a 'mere direction of the attention upon' a process differ from an 'analytic attention' to it? Why should the two forms or modes of attention lead to diametrically opposed affective results? I have committed myself to the view that attention, in the first case, is a passive attention to the sensible substrate of the feeling, while in the second case it is a baffled or misdirected active attention. The view is tentative, but it has at least the advantage of setting the two groups of facts in systematic relation. And I see no reason, in anything that they have so far written, why Zoneff and Meumann should not accept it.

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ON McDOUGALL'S OBSERVATIONS REGARDING LIGHT- AND COLOR-VISION.

Mr. W. McDougall published in the January, April and July numbers of *Mind*, 1901, three papers entitled: 'Some New Observations in Support of Thomas Young's Theory of Light and Colour Vision.' Mr. McDougall there described some fifty experiments which he had made, and drew certain conclusions from them. On the basis of these observations he would reject the Hering-Müller theory of light- and color-vision and substitute for it a modified form of Thomas Young's theory. Mr. McDougall's observations present an extensive array of ingenious devices and of suggestive facts, but in the interpretation of such an amount of material it is perhaps inevitable that some

few points should be called in question. The purpose of this comment is to suggest:

(*a*) That these experiments do not in every particular warrant the conclusions drawn from them.

(*b*) That there are certain facts which offer difficulties for the Young-McDougall theory.

(*c*) A general reason why the Hering-Müller type of theory is preferable to the other.

Mr. McDougall's position is outlined in what follows. He begins with the remark, 'that all writers assume that the physiological and psychological processes in the visual areas of the cortex run exactly parallel to retinal processes.' What can such a statement mean? Is not every act of selective attention among visual stimuli a case where cortical and retinal processes are not exactly parallel? Are not visual reflexes one kind of instance, and subliminal stimulations another, in which peripheral excitation and consciousness are disparate? Yet what is commoner or more obvious? What does Mr. McDougall suppose that psychologists mean by the distinction of central and peripheral? Surely this difference is not a new observation. However, this disparity of cerebral and retinal process is illustrated at some length, and this is, he says, the basis of his theory.

On the retinal side Mr. McDougall assumes that there are four exciting substances, which he calls the red, green, blue and white (R, G, B, and W) *x*-substances. When light acts upon the retina it frees (by decomposition) these substances, and by a further chemical process (combination) they each excite their corresponding nerve endings, that is, the termini of four distinct color-systems, R, G, B and W cerebro-retinal systems. Light of any color sets free all four kinds of substances but acts most vigorously on one; thus R light frees B, G and W, but it frees R in greater quantity, hence the predominance of red sensation. These substances are highly diffusible, so that when they are freed in any definite part of the retina they tend to diffuse themselves into surrounding areas and there give rise to sensations.

This is the entire retinal basis for the phenomena of light and color vision; all variations not referable to these processes must be explained in cerebral terms. Thus yellow sensation is a psychical fusion of red and green, and the experience black is the inhibition or lack of sensation.

The particular point at which the Hering theory (and Müller's as well) is attacked is in the discussion of the facts of simultaneous and successive contrast and induction. The case of white light is ex-

plained first. In *simultaneous induction* the directly stimulated area of the retina diffuses the *x*-substances into adjoining parts and they there give rise to sensations. *Successive induction* is the persistence of white *x*-substances in those areas. In *simultaneous contrast* the cortical processes excited by a *W* patch of light tend to inhibit all those from the rest of the visual field. *Successive contrast* is the case where the cortical processes excited by *x*-substances diffused into outside areas come to predominate over those aroused by the directly stimulated part, that is, where the after-image appears as a white halo around a black area.

Black sensation is in each case the result of cortical inhibition.

He then goes on to explain all contrasts and after-images of colored lights in terms of the cortex. The principle underlying color-contrast is that W being the resultant of the simultaneous activity of the R, G, B color-systems, the inhibition of W by W involves the inhibition of R by R, B by B and of G by G. Now if the retina be stimulated by a patch of red on a gray background, the R tends much more strongly to inhibit the R in the gray ground than the B, G, which the patch of red contains tends to inhibit the B, G of the ground, hence the effect of simultaneous contrast. Successive contrast, or the case of the complementarily colored after-image, is explained by cortical changes, and simultaneous and successive induction or colored light are closely analogous to those phenomena in the case of white light.

Inhibition with Mr. McDougall means always cortical inhibition, but for R, G, B and W he allows a retinal origin which he denies to black. This brings us to the principal difference between the Hering and McDougall theories, which is that Hering assigns a special retinal process as the correlative of the sensation black, whereas Mr. McDougall maintains that no such independent process exists and that the physiological basis of black sensation is simply cortical inhibition.

In support of his general position Mr. McDougall makes a very interesting and exhaustive study of all forms of retinal rivalry, and he states most ably the important part played by the cortex in light- and color-vision. In connection with details of his theory, however, he adduces, among others, the following experiments:

1. See Observations 16, 17, 18 and 19, Article I. The discussion here is whether the relation of black to white is the same as the relation of other colors to their complementaries in the matter of successive contrast or the negative after-image. The first observation was made on a disc white at its center and shading gradually out to black against a black ground. After fixating this disc of shaded black and

white no negative after-image, only a positive one, was seen. After this colored discs constructed in the same way were used, for example, a disc with a blue center shading gradually out to black. In this case a negative after-image was observed. This experiment is taken as evidence that black cannot excite an after-image of complementary tone in the way that other colors excite their complementaries, but it is obvious that for such a demonstration this experiment cannot be correctly organized. Mr. McDougall is comparing the reactions of different pairs of colors, and if, as the first member of the comparison, he takes a disc shading by many gradations between the complementaries white and black, he ought logically to take as his second case a disc, say, of blue gradually shading into yellow, or of red merging by degrees into green. If he wants to compare black with other colors he must take them under like conditions. For the purpose of such a comparison it does not have the slightest point to contrast black shading up to white, with blue shading off to black.

2. Mr. McDougall wishes to prove that the after-image and the direct image have their seat in the same part of the cerebro-retinal system. He fixated for a few seconds a patch of red light (Obs. 9) and then planted its green after-image beside the red patch. The red and green then faded and reappeared together. This proves, he says, that they are affected in the same way by the same factors, and that the seat of the after-image is in the retina.

A little farther on (Obs. 14) Mr. McDougall showed that an after-image may be inhibited by a direct image on a different part of the same retina. Why not say that this proves that the two are not affected in the same way by the same factors, and that the after-image is, therefore, not in the retina? It seems curious that Mr. McDougall should not bring in other available evidence if he thinks the retinal location of the after-image requires proof. He might quote not only the experimental results of Exner, but certain striking facts of common observation, *e. g.*, that the after-image follows every movement of the eye-ball.

3. Yellow sensation, he holds, is due to the psychical fusion of red and green. This is proved by the facts: (*a*) That the mixture of red and green give a yellow of good saturation (Obs. 49); (*b*) that the cycle of color in the after-image of yellow shows markedly a red and a green phase (Obs. 41). But what does Mr. McDougall say to the fact that in peripheral color-vision we can see yellow with elements of the retina which are absolutely insensitive to the difference of red and green?

In this connection he quotes Mrs. Franklin's criticism on the theories of Gegenfarben, in which she says that the red and green, which subjectively are most saturated, are not complementary (antagonistic), but that they produce yellow. But this observation gives no countenance to a psychic fusion theory of yellow, since we have in Mrs. Franklin's own words a more plausible explanation in retinal terms: "Blue and yellow are complementary colors, but red and green, when acting in conjunction, re-compose the yellow-producing substance out of which they have been developed, instead of together making white" (PSYCHOLOGICAL REVIEW, January, 1899).

4. On the basis of one experiment (Obs. 22) Mr. McDougall disputes the fact of the simultaneous contrast effect of black. Against this position we may quote the results of Hering, Mach, Sherrington and others. In the following simple test it seems scarcely possible that one should fail to get this effect.

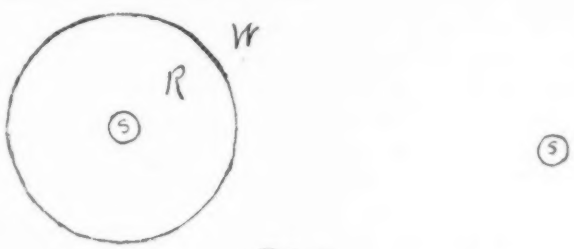

FIG. 1.

On a white ground (Fig. 1) lies a circular red patch three centimeters in diameter with a black spot at the center. Fixate the black spot for five or six seconds and the red will begin to show dark at the edges and bright red immediately around the spot, while between the dark and the bright red there still lies a broad ring of the unaffected color. Now transfer the after-image to a white ground and center it on a black spot like the first one. In a few seconds a rim of brilliant white light appears to radiate from around the black spot.

5. In the case of the binocular combination of color Mr. McDougall rests in absolute security. Here the fusion seems unquestionably to be a psychic one; but in view of the strong sympathetic connection, upon which he dwells, between the cerebro-retinal systems of the two eyes, is there not a retinal explanation at least possible even here? We may conceive that the blue which excites one retina finds a slight response in the other, while the red which is acting on the second retina is echoed in the first, so that the resulting purple is after all based upon retinal fusion.

6. In observation 24 there is reproduced an experiment of Müller's, in which dark red against a black ground is compared with the same red against a white ground.

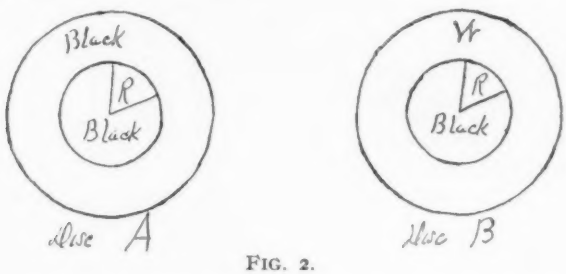

FIG. 2.

Upon rotating the two discs (Fig. 2) the red of *A* appears more saturated than the red of *B*. One would suppose that the result might be explained as a mixture of red with contrast white in disc *A* or a mixture of red with contrast black in disc *B*; but Mr. McDougall rejects such a view, without giving any definite reason, and he says that in disk *B* the white of the background inhibits the white which the red light contains and hence makes that red still darker.

Many of these experiments are extremely interesting and valuable in themselves, but one could scarcely agree that they support unequivocally any theory which has been advanced in these papers. In fact the conclusions seem often a trifle premature and the above citations serve to illustrate that some of them are logically a little surprising.

If Mr. McDougall is to maintain the theory that there is no black-exciting process in the retina, but that black is the inhibition or absence of sensation, he has still one or two questions to answer:

1. If contrast black is the result of suppression of faint gray light by a bright white light, that is, if it is a case of cortical inhibition, why does it persist when we expressly attend to that part of the field in which it appears, why does the faint gray light not emerge into consciousness?

2. If black is mere lack of sensation, why do we not see black with the blind spot in monocular vision?

3. If black is a negative element, why does a mixture of black with other colors change not only their saturation, but their color-tone as well.

4. Subjectively, black is not a 'neutral' color, nor is gray a mere diminution of white; gray appears to us something qualitatively very different from white, like the mixture of some positive element with white. The following observation may touch this point: In mixing

complementaries on the color-wheel, if the colors differ in luminosity so that the flicker is pronounced, *e. g.*, in blue and yellow, it is noticed that the field suddenly assumes a rough resemblance to a black-and-white checker-board. Instead of the two colors changing at once into an even medium gray the process of mixture takes on a very decided black-and-white phase, suggesting that the resulting gray is due to the stimulation of two distinct retinal elements.

The tendency throughout Mr. McDougall's papers is to emphasize the part played in light- and color-vision by the cortex. Whenever possible he explains in terms of cortical attention, and he apparently feels that it is a commendable simplicity in his theory to assume as few retinal processes as possible and to refer all further variations to purely central fusions and suppressions.

Attention seems to be the refuge from a great many psychological tangles, since there is nothing which it cannot be made to explain. The fluctuation of attention is the very basis of our psychic life, it is the fundamental problem which holds the others in solution, and it is just on this account that it ought to be avoided as an explanation. No one has a right to resort to an ultimate principle until all hope of a more special explanation has failed. The tendency of scientific theory is to pass from the vague generality to the specific cause, and in dealing with sense-discrimination it has moved steadily away from the explanation in cortical terms towards that in peripheral terms. It is a more exact and intelligible mode of thought to conceive of sense differences as determined by structural differences in the sense-organs, than to suppose that they are perceived by some deft and mysterious intelligence in the cortex. Although no general considerations of this sort can prove or disprove any particular theory, yet they can lay out the direction of progress for that theory.

In comparing the retinal theories of Hering, Müller, Franklin, with the psychical of Young-McDougall, we may say that the former are forward-moving theories and the latter is a step backward. Mr. McDougall's method may be thoroughly safe, but it will never be illuminating. The Hering theory as a type is in the line of progress, and although there may be difficulties with its precise formulation it must still be the preferable hypothesis.

Mr. McDougall's criticisms have a positive value in searching so minutely as they do the frailties of the Hering theory, and in reminding psychologists of the inadequacy of any color-theory which has yet been proposed.

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SOME CHARACTERISTICS OF THE GENETIC METHOD.[1]

It may be readily confessed that the following observations arose in response to a personal want and interest. While engaged in a wider topic, the inquiries failed to discover any thoroughgoing presentation and discussion of this particular theme by psychologists. The conception of genesis has become overshadowing to all types of current science. The adaptation of method to this conception has been attempted repeatedly, and sketches of the character of the result in method have been given here and there, but not in a thoroughly satisfying manner. These reflections are not set forth as answering all the demands of that personal want and interest, but rather with the hope that they encompass the field with some system and penetration.

Our inquiry, then, is not to be merely a census among psychological methods, but rather how best psychology can create its problems, and how it can be sponsor for true problems of science. As soon as a science ventures beyond a dogmatic attitude—and to be dogmatic is not an impossible quality for science!—it is immediately confronted with the question of method. By method, we must understand the fullest sense of the term. It is not the mere pedagogical preliminary, which almost every text-book and other exposition of the science implies that it is. These well-nigh invariably thrust the treatment of method into the foreground. All this involves the assumption that method is a very minor matter, being only the question of procedure, which can be described and settled once and for all, and then immediately forgotten.

The usual text-book notion of method in psychology regards it as something quite splitable. There are the direct and the indirect methods; the subjective and the objective; the individual and the collective; the logical and the genetic; the experimental and the enumerative; the analytical and the synthetical; the historical and the projective; human and comparative—and there might be more of them if only our language were rich enough in adjectives! These methods are such, however, as it is often said, only in light of the data which are the peculiar property of psychology—just as though a fact of soul life is so completely and so variously accessible to the gaze of the inquirer! Or, these varieties of method are also taken to mean that no single soul fact recurs in the acquisitions of any two or more methods, but appears only once, and in the net returns of but one given method—

[1] Read by title before the Tenth Annual Meeting of the American Psychological Association, December, 1901, University of Chicago.

just as though the sum of methods should be regarded as the sum of soul facts! Undoubtedly there is a valuable suggestion as to the well-nigh inexhaustible richness of mental experience in this array of methods as forms of procedure, all of which are to be applied before the student can lay claim to possessing anything more than a mere outline or elementary scheme of a reasoned account of 'inner experience.' If psychology has shown more or less surely that the soul can be gotten at by such an indefinite number of modes of approach, that fact, as a mere item in human civilization, is worth all the multitudinous efforts required to establish it. At the same time, there is not the most credulous of us moderns who for a moment believes that a grab-baggy, drag-netty *potpourri* ever constitutes a science. To lay bare the soul, to expose its intimate transactions, is the business of life, whether in the career of the individual, the activity of a pupil in the school, or the issues of the hero in the scheme of art and literature. Science may represent life, but does not manufacture it. If not, then the completest and minutest biography would be the acme of the science of the soul.

If these strictures upon current notions of method—and they apply *mutatis mutandis* to every other type of science besides that of psychology—be true and fitting, we may find here an antithesis which may well venture to become an antinomy in our most desperate efforts to force psychological science to square itself with both method of thought and content of experience in thorough and unquestioned conclusion as to what our real knowledge of soul life shall be.

The truer conception of scientific method, while not denying its more limited pedagogical function as just indicated, requires that we conceive the question of method as almost identical with the question of the standpoint, yea, of the science itself. In no field of modern inquiry is this larger conception more appropriate to the nature of the subject-matter and more indicative of the scientific scope than in the case of psychology. This truth has often been inadvertently admitted in the many early discussions about 'introspection,' where the question of method confessedly became a question of the possibility of the science. With all our marvellous advances in instrumentation and tabulation, the very continuity of psychology as a widening field of human activity is still dependent upon the alternative which grew out of those discussions: either introspect, or give up the science. The standpoint of interpretation, however, stands or falls with this outcome, all of which is summed up in the final question: How are we going *to think* in psychology?

This does not necessarily imply that there is a mathematical thinking, a physical thinking, a chemical thinking, a biological thinking, each of which, or all of which, may be different from psychological thinking. For, all science is produced by human thought, in fact *is* human thought. And yet, the psychologist in mathematics, for example, presents the spectacle of the proverbial bull in the china shop! Even so is it with the chemist in psychology, or with any other form of 'cross-breeding' among the departments of science and the devotees of science. Apperception, both scientifically and psychologically, may thus appear to be a very demon, tormenting us with all the incredulities and monstrosities of an integral science.

The first suggestion arising from our topic is twofold: first, that method is an external feature of science—its means, which may be extremely variable, to an end of verified knowledge of given or natural objects; and second, that there may be more than one method in psychological science. These two suggestions are at bottom one and the same, namely, that manual dexterity and the thinking which enter into the constitution of a science are radically different. This implies that any sort of a skill in attacking an unknown region of facts might unquestionably go along in company with any sort of an acceptable theoretical construction of such facts. It undermines the notion that the specific scientist himself should be fundamentally interested in and concerned with the formulation of the method which he is to 'apply.' It is in contradiction with the actual experiences of psychologists, and fails to throw any light upon the progress of psychology, whether its standpoint has been metaphysical or more modernly positivistic. It disregards that trait of investigation which is expressed in the necessity of dealing with facts after they have once been elevated out of the *milieu* of experience, which, to be consequential, must be present in the very beginning of the methodical procedure. Thus, and as could well be indicated in other ways, this suggestion is barren of fruit in offering a clue to the proper regard of the relation of attitude and the execution of scientific thought. This is particularly the case in psychology, where no sort of subterfuge in attacking the domain of conscious facts, whether by mere enumeration, or reaction sorting, or brain division, or concept perfection, can by any possible means bring about the conquering leap over the walls of experience which have been assailed by all the indefinite variety of machines devised for the warfare of scientific method. Thus far our considerations have been negative, leaving upon our hands the strong intimation that there is apparent reason for a continual and basal cleavage between psychological science and its method.

Coming to our topic, we may ask, what is the 'genetic' method? What are its *differentiæ?* How does it get 'applied'? What does it take for granted? What are its results? We can probably find no uniform opinion among the psychologists as to what the method really is. And, if we turn to the uses which have been made and are being made of it, our inquiry would probably be rewarded with a discovery of still greater divergence among them. Of the fact that there is a widening dependence upon this method, one can readily be assured by a hasty glance at any one of the various psychological indexes which are available annually, or at the end of every text-book chapter written now-a-days. This disparity of usage and the persistency of mutual criticism among the methods produces a feeling of despair, which finds some relief, at least, if no definite solution, by dashing off a host of questions which come trooping up as soon as it is proposed to state what the method is. What is 'genesis' anyway? What can we mean by 'psychological' genesis? Is genesis more a matter of time and history, or of analysis and logic? Can the genetic standpoint ever delimit itself with any sort of analytical precision? If one can speak of mental genesis, how is this to be distinguished from physical genesis? Why is it incumbent upon psychology to go outside of consciousness in order to get a scientific, explanatory principle? Can the genetic method offer any valid basis for the classification of data? Whence the push which is said to be revealed in mental 'unfolding'? Can the method recognize any 'content' in consciousness? Is it not compelled to disregard everything except processes—presuming, of course, that there might be anything else in the mind? How and why does the soul make itself to become what it is? Or, does something else make the soul as we find it to be? Is there anything immanent in soul growth? These questions, it is true, are not usually put in these terms, and often they are not mentioned at all as representing the real problem of the genetic method. Yet they may be regarded as offering real aspects of the great problem to the solution of which we may conceive the method as setting itself.

To be in accord with the spirit of its application, one would be compelled to believe that the genetic method is but a dawning of the latest researches. To be true to the historic facts, however, one finds that the birth of the science lay in the application of the method itself —or, at least of *a* genetic method—to the facts of soul life. Both the science and the method had their coördinate beginnings with Aristotle's attempt to approach the soul of man. If we also look at the modern era (without attempting to be exhaustive and systematic), we

see that Waitz in 1852 conceived of psychology as both comparative and genetic. The next year Morrell depicted stages of development among the 'faculties' themselves. In 1855, Spencer's attempt with the method became a supreme application of it, with all fidelity to its peculiar logic. Twenty years later Volkmann wrote his two splendid volumes 'from the standpoint of realism according to the genetic method.' Preyer worked industriously from the morphological point of view. Or, within the last decade, Ladd makes an application of the method to the specific forms of mental life in man. Stout attempts to apply the method on the ground that 'the individual consciousness is but a fragment of the general system of the world,' and Baldwin sets off the method as pursuing distinct problems after the work of analysis seems to have been done.[1]

The historical potency of the problem of psychogenesis as determining the dominant method of psychology would be radically missed, aside from the bare historical data of the appearance of the concept in scientific consciousness, if no attention were paid to the introduction and use of the term 'function' in all discussions pertaining to consciousness (not to the physiological issues). The Herbartian dethronement of the 'faculty' concept made way for the more modern aspirant of 'function' (which was, indeed, vigorously promoted by Herbart's own use of *Vorstellung*). Indeed, the term 'function' implies the genetic method; and perhaps it is the great achievement of this standpoint to have focused attention upon function as an object of analysis which could hopefully resolve the confusion imminent in all treatment of mind and brain. Lewes was probably too conciliatory for real genetic progress, when he suggested that 'function' should mean 'endowment,' and 'faculty,' 'variation.' Steadily since, however, function is the psychological slogan of such warriors as Ward, James and Stout. One biologist recently so far forgot the genesis of this conception of science as to affirm unblushingly that consciousness as a function came before there was ever an organized brain! Thus one can sketch the wide-spreading, silent, and at times uncertain intrusion of the genetic spirit into our science.

We approach the intimate nature of the genetic method if we turn to notice first, as one of its peculiar characteristics, the material out of which it proposes to build the science of mind and the material of consciousness which is to receive recognition from its hand. It pre-

[1] These and later references are not to be regarded as 'demonstrations' aiding the examination; they are mentioned more as conservative instances, and not as types indicative of variations in the method.

supposes the work of analysis as being more or less completed, and in part on the basis of these results attempts to build up its own constructions. It is extremely selective in accepting the material with which it works. Not *all* soul facts are employed in working out its great generalizations. It endeavors to account for all the functions of consciousness known to the method of analysis, by referring them to so-called lower grades of processes. In dealing with the lowest 'forms' of conscious action, such as 'feeling,' 'instinct,' automatic processes, etc., genetic psychology violently differentiates itself from the other methods. That material of psychology which bears the marks of being the most persistent, ubiquitous, and hence most 'fundamental' in the economy of the soul as a product of nature, is said to be the special field in which this method plies its industry.

Closely connected with this feature of material is the dominant conception which overshadows its elaboration. When we regard the soul as a 'thing,' the method of deduction is alone appropriate to meet the demands of both science and its material. When we regard the soul as a 'process,' the inductive method alone can be adapted to the content of that experience to which all psychology should in every case refer. It is only as we combine these views of the soul that we have brought ourselves into a position where the question of genesis is said to force itself paramountly upon our attention. It is only when we think of the soul as in a process of development, 'unfoldment,' if one please, or even as that development itself and nothing more or less, that the genetic method is made possible, with all its reconstructive force in the sciences which are vitally concerned in displaying truth about that soul life. The 'process' and the 'thing' views must be unified in a conception that regards the thing not as static, nor the process as merely drifting; then we first come to a right view concerning the object of psychological research. Extremists, however, readily contend that the genetic method can accept neither the thing nor the process view of the soul, and that they are mutually exclusive. And, indeed, it is just against this 'misuse' of the genetic method that we would offer our solemn protest.

The remaining question is that of *how* we shall regard the soul as developing: whether individually, cerebrally, or racially. No single discussion can settle this dispute, which will go on indefinitely. But it can at least put its finger upon the position this issue holds with respect to the general features of the method as a whole. But here method is seen again to mean essentially the principles of interpretation.

Other features of the genetic method appear, secondly, if we compare it with two or three generic lines of current psychological thought. It apparently tends to do away with brain psychology. In fact, it would seem at first blush that the inheritance of soul portions, a confessed feature of mind according to the method of genesis, could be described irrespective of the empirical relations the soul may sustain to cerebral structure and function. The fair hope of a consciousness psychology seems now almost fully within our grasp. But this is only the first impression of the relation between a brain and a genetic psychology. For, no sooner have the genetic functions been sorted out in consciousness than the reconstruction of them into the working processes in consciousness must trace their progressive synthesis up through the aggregation of ganglia, which finally produce the brain. Whether the genetic method can ever get beyond this point of view, and be able consistently to regard consciousness as a 'natural' object, yet somehow not totally dependent upon the brain, is a very serious and difficult question. To the biologist, of course, such a question means nothing. His point of view demands nothing more than cell colonization!

There is also a very positive way in which the genetic method is but brain psychology thinly disguised. For, cerebral morphology is explored, as well as comparative morphology, as containing the infallible transcript of the order of psychological precedence. Definite organ, so the credo would run, stands for definite function. All this would have to be taken with a meaning much stronger than that of a mere analogical parallelism.

When contrasted with the stimulus psychology—which is probably more fundamental from the standpoint of collective science than any other form of objective psychology, and especially more so than a brain psychology because of its extreme approximation to accuracy in the possibilities of measuring in definite terms the causal quantities of stimuli—the genetic psychology differentiates itself both by its mode of 'explanation' and by the material of the soul life which it accepts as needing explanation. It is one of the unique features of science that there is a logical connection between its methods and the materials which are accredited for study; and this comes out in this present contrast. Stimulus psychology attempts to account for the origin of the content of the individual mind by going directly to physical nature; genetic psychology struggles with the material which can be connected with ancestral genesis.

One of the most interesting aspects of the genetic method appears

in a farther contrast of it with the experimental method. Here we have, indeed, the two progressive rivals for the control of the entire field of psychology. The earlier claimant is that of genesis; but the more widely applied is that of experiment. The latter, too, has enriched the content of known truths about the soul and conditions of its activity more largely than the former. This is due, of course, to the fact that experiment is generously taken to include not only a stimulus, but an enumeration psychology. The weakness of the genetic, and the superiority of the experimental method are at once seen in those attempts at a use of the latter in the interest of the former. No *single* experiment and no complicated series of *experiments* can ever succeed in demonstrating the fact of genesis or in picking out any given stage in its processes. For, an experiment gives us a 'dead' result which, by its own method of ascertainment, cannot be woven into an elementary connection with genetic data. Geneticists may endeavor to employ experiment; but this offers 'results' only when the particular hypothesis of development has been first elaborated. Comparison with a special intent alone can turn the data of experiment into support for the hypothesis. This limitation of the genetic method, and this antithesis between these two types of method, are even more marked if the subject of experimental reaction is selected from the animals or from the period of infancy. Thus it is made clear that the genetic method is primarily not a method of obtaining data and validating judgments about them, but a method of inference in construing such and, perhaps, all other data into an ideal scheme.

As a result of all these features, the genetic method comes forward with its characteristic claim that it alone does entirely away with the radical distinction between the method and the content of a science. From the standpoint of genesis, these are to be identified. The order of exposition is to be the order of development, and the order of development is to be the order of exposition and of scientific discovery. The progress of thought on the way to knowledge is to repeat the progress which in fact nature presents in this case, the 'psychological' aspects of all nature. Here, then, is the supposed unity: the development is the science, and the science is the development.

Method, however, in this application first means mere order of development, and a truthful arrangement in thought is held to be that which copies the earlier arrangement which nature exhibits. This attempt at unification is also less a feature of method, than it is a matter of postulation. Here we find one of the limitations of the genetic method, and also one of its good features, which, namely, meets in a

unique way our earlier demand that method shall mean standpoint and not mere means.

A third and most marked characteristic of the genetic method is to be found in the claim made for it, that it offers a way of escape with regard to the question of *psychological causation*. As any student of psychological problems in any period of discussion readily discovers, the most vexing of these belongs to this very item, whether 'causation' shall be admitted in psychological theory; and, if so, how it shall be construed. Now it is affirmed, and then it is denied, that mental states can really be viewed as standing in a causal *nexus*. Witness the old-time debates about psychological motivation and freedom, and the but lately heated disputes over the scope and conditions of mind and brain parallelism. And that very question of our science itself: Is it right, proper, and within our power to have any knowledge of the mind which shall receive its support and scientific appeal solely in that type of explanation composed only of admittedly distinct mental factors? or, can we lay claim to a psychology only by deriving its foundations in the physical and chemical processes given immediately in the body, and diligently studied by all stimuli psychologists, on the one hand, and by appealing to a great so-called prototype of psychical changes which is given in the phenomena of life-genesis and is diligently studied by all biological psychologists, on the other? And one might go on to indicate other fundamental tendencies in psychology, all of which come back eventually to this query: What are the great terms of the mental life, and under what specific relation must they be handled before we can accredit the results of the science? This is, indeed, bringing into question, in an altogether negative and condemning way, the existence and the right to exist, of this science. And yet the psychologists fortuitously labor to increase our knowledge of mind, and there is apparent continuity in the progression of their specific and generic achievements!

I do not think this presents the situation too seriously, and the pivot on which turns the self-equilibration of psychology, as well as the acceptance of psychology by the self-constituted committee of award made up of those scientists already in the possession of the field of nature, is this matter of causation within consciousness.

The genetic method fancies itself seeing the impossibility of ever ending this debate on the grounds of its own assumptions, and rushes forward with a substitute conception. To this method, continual debate is conclusive evidence of error. The substitute eliminates the whole item of psychological causation, and, thus robbing the disput-

ants of their soaring inflatus, would let them fall to earth, dashing both themselves and their meaningless contentions to pieces. Indeed, the genetic method would do away with all 'causation' in psychology. This *nexus*, it is affirmed, is perfectly well known in the physical world, but is misleading and confusing when introduced into the mental world. This may essentially involve a monadism which is more or less distasteful to all psychology as such, but which is not disturbing to the method in its mission of philanthropy.

In observing this attempt to settle the old-time causation dispute in psychology by simply removing it, we come upon one of the fundamental characteristics which the genetic method displays, namely, its struggle to fill the void created by that removal. The method has no particular quarrel with causation as one of the working categories; it only struggles to put its own interpretation upon such a category. This it does by introducing the conception of 'stages' and their one relation, which is said to be 'genetic.' These stages offer an easy picture for the exercise of the scientific imagination. These stages are related only in time, and they stand in the order of succession. They are not toy stages, subject to the indiscriminate manipulations of the child playing with them; for this time order is made to do service in that great causal void. These 'stages' are 'lower' and 'higher,' and there is a necessary (*sic*) arrangement among them, both in nature and in the representing science. The 'higher' stage is later in time, and is conceived of as 'fully explained' by the next lower stage. Instead of cause and effect, the method pursues lower and higher stages, and instead of causal connection, it thinks only of stage transitions and successions.

To this characteristic dealing with simple 'stages,' there must be added the ramifications of the genetic conception in order to make it workable. There is psychological 'nascency,' psychological 'blooming,' and psychological 'decadence.' The aggregate of these is bunched into psychological 'recapitulation.' Individual stages may be 'furthered,' 'arrested,' 'appear' and 'disappear.' The effort is made to 'explain' the ages of the soul, rather than its fundamental or its incidental 'processes' as a given object in nature. Time is here made the great soul divider, forcing a cleavage which no efforts can ever be successful in reuniting. The standpoint of the method herein is practically an assumption, not of the uniformity of 'processes' in the individual mind, but rather of the uniformity of racial scintillations within the individual. In this way the individual conciousness, as known to or conceived of by the other forms of psychological thought,

is literally pushed aside, or 'transcended,' in order to get at the soul as a denizen of the world. Thus we see something of a logical sequence in the emphasis which the method places upon the *differentiæ* of eras and epochs in the individual, rather than upon the continuity of process in the individual consciousness.

The instruments known to psychological analysts, such as faculty, idea, conscious process, sensation, and so forth, are thus disposed of at almost one stroke. All those efforts of the genetic method are not to be identified or confused with 'psychological history,' which must always be limited to the individual mind. At the same time, an historical usage is made of the 'explanatory' coin of the method by having it pay for an archæological journey into the soul's domain, there viewing the slow and 'conscious' acquisitions which have preserved 'themselves'—but not their identity—in 'the neural and automatic and instinctive and unconscious processes' so readily traceable, as it is affirmed, in the geography of the soul of to-day. The 'over-soul' of a metaphysical psychology, even the common-place, observable soul of analytical psychology, is not known in this historical tracing. By such a long detour, the method of genesis endeavors to 'get into' the soul of the present day, by maintaining that other methods do not 'get into' the soul at all. Thus, psychology is only one of the racial means for aiding the soul to get inside of itself!

Are we to believe that the 'lower' stage, when ascertained, 'explains' the stage next 'higher,' when it becomes known? Are these stages static units, standing in the developing progression, which can be picked out and treated as units, even as the abstracted units of scientific thought? What but the 'higher' stage can determine what and which the 'lower' is? And, when it is demanded that our psychological quest shall be for 'stages,' then our science must frankly confess that it can never rise above the merely descriptive plane, and also that through the genius of genesis it has virtually fallen back into that estate which had been established unto it by the methods of speculative abstraction of a century ago.

This assumption—or perhaps, as truer to the facts, this unsecured loan from the biological scheme—that soul facts are self-explanatory when they are arranged in a 'genetic' order, remains wholly without an exhaustive examination. The logic of scientific judgments, however, cannot by any possible dialectic be identified with the morphological push to the genetic method on the grounds of mere science. Herein we have brought home to us a strong suspicion that the truth of the matter will probably be disclosed in the fact that the genetic

method is merely an exponent of a 'genetic' philosophy. Psychology is here under the constraining invitation to return to its earlier metaphysical imprisonment!

Suppose we have reason to hold, on the other hand, that real explanation—that something which alone can make our psychology a veritable science of mind—works in a direction directly opposite to that which evolution is said to pursue. Suppose it is held, in truth, that the higher 'stage' is first able to throw light upon, and to give meaning to, its predecessors. Such a view leaves abundant room for the genetic method to ply its profession constructively in a study of mind; but it does not dispose itself to allow the postulates of a method to displace the final necessity of specific intellectual activity being directed towards specific problems which confront one for 'explanation.'

Another defect in this doctrine of 'self-explanatory' stages is to be found in the violent contradiction between it and the practice of the geneticists. This practice takes its twofold foundation in the conception of *reflex action* (physiologically), and in the effort to trace all mental content from *one* specific, primordial *quality* (psychologically), such as mere sensation or mere feeling. The theory of this practice maintains that whatever the soul may be, it is naught but a variant of the original function, and that whatever the soul may do, is of the nature of a reflex, derived either from the proddings of a world environment, or from the elevation of the 'remains' of psychological ancestry—for it is not said there are no soul worms to destroy that ancestry—which are lugged into the precincts of the individual mind by the laws of soul heredity. Here we have a nest of incoherencies, which would not be permitted to prevail a moment in any 'natural' science other than psychology.

In the fourth place, we find that the genetic method is sometimes fondly regarded as the final and the highest method of psychology. This relative rank is put forward as one of its chief characteristics, and as one which therefore entitles it to the highest respect and to a throughgoing adoption by the science. *Ne plus ultra* is a fine thing to say of a method, and there are those among us who find in the tracery of beginnings the final outcome of one's thoughts about the soul. This practically says that, when in the spirit of this method, we traverse the country of the soul, we shall have exhausted its content and accordingly brought the science to its perfection. No more problems are then to remain. Idyllic contemplation will then sublimate all psychological work into æsthetic repose!

In reply to this great claim for the genetic method, it must be

emphatically stated that the pristine psychological method was the genetic, which was so remarkably applied by Aristotle, the founder of the science. To him, however, it was not the final goal of the science, but only a very convenient external scaffolding, which he rejected for his later penetrating analysis of reason. This mere historic precedence of the method may, however, leave entirely open the question of its validity for psychology; and its neglect from the time of its projector until the last century, to be sure, cannot be forced into an argument against its validity. The claim that it mounts above all other methods receives in this historic fact a severe shock, inasmuch as the definite progression of methods preceding its conception and application, demanded by the claim in order to be valid, is found to be lacking in the case of our great founder.

In the second place, it may be replied to this claim that, instead of bringing peace, it has brought the sword to psychology. Instead of introducing apparent unity and progress towards perfection in the science, the genetic method has thus far succeeded only in breaking up the subject and in scattering forces. Since 1855, the date of its extreme application, the method in its results has had a fitful history. This fitfulness can hardly be looked upon as a mere consequence of the number of 'new' problems which are possible from the genetic standpoint. Its adherents may, indeed, regard this condition as necessary and desirable before the method can rightfully come into its own. They may explain this turmoil as merely the inevitable progression from disparate judgments to the unity that is to come. This excuse for the effects of the method may be accepted for all that it is worth, while at the same time one must whisper a doubt whether the growth of a science is really advanced by revolution, which this method fosters.

The problem of genesis is a worthy problem, academic though it may at first appear to be, especially when the genesis is that of the human soul. The worthiness of the problem can be seen in its riper fruits in the psychology of the child, of the animal and of the race, which have grown under the inspiration of the genetic conception, but not as a result of the application of that method. These genetic branches of psychology have indeed flourished on the basis of the adaptation of particular methods of the science, to which that of introspection has always been ancillary. We must thus not fail to make the radical distinction between genesis as *one* of the veritable problems which we should raise concerning mind, and genesis as the *sole* problem of psychology, in the light of which the entire science is to be

reconstructed. The latter carries the identification of problem, method, and resultant science to an extreme. And often the last extremity of credulity is played upon in so doing, as may readily be seen if one attempts to catalogue some of the assumptions which support the method.

As a fifth group of characteristics of the genetic method, I herewith venture to set down, without accompanying discussion, some of the assumptions which envelope the standpoint of the method, giving it all the force to its arguments which they can possess. It is not meant to be implied that these assumptions are gratuitous and independent of the empirical necessities of a scientific adjustment of mental data.

The genetic method assumes that there is one or more definitive relations between mental states as they appear in the life of the individual mind, but not as defined by the limits of that one mind.

It assumes a 'racial' consciousness, which parallels that of the individual and, in fact, underlies that of the individual. From this unconscious background or underground to consciousness many of the features of genetic hypotheses are derived. While assuming the play of the environment of a psychological order as all-important in empirical explanation, this environment is sublimated by the heredity of soul, which forces one stage to succeed another irrespective of environment.

Prefacing the work of analysis as completely finished, it assumes 'feeling' in some one of its specific variants to be the primitive consciousness, both in the biological series of the cosmic order and in the morphological series which constructs the individual.

Along with the assumption of stages in the individual mind, which are residual of large eras of ancestral experiences, not necessarily human, goes the corollary assumption of psychological heredity and the inheritance of acquired characteristics, by means of which alone the alleged continuity of mind life, linking all individual souls into one unbroken series, is capable of breaking over the limits of the dead-level of heredity and providing for the manifest accretions to later forms of mental experience.

In going outside the individual's consciousness, *i. e.*, outside the individual organism, to get its explanatory basis and data, it adopts the extreme validity of analogy as the constructive method of science, in so far as it ventures to deal with the mind as a distinct object in the series of nature.

Such a catalogue of assumptions might be slightly increased, but

it seems to represent substantially just the status of psychological genesis when it is conceived of as the sole problem of psychology.

No discussion of the evolutionary reach of psychology would be at all adequate if it failed to treat of the probability which characterizes its final judgments. As we all know, the primary danger in psychology is that of accepting objective error, and not any subjective harm. And, indeed, the progress of any particular science can be marked better by the steps it has taken to eliminate its errors, and to increase the degree of probability which attaches to its conclusions, than by any other mode of tracing the activity within the science. In estimating the presence and scope of error, however, we must not fail to make the radical distinction between the mere fact of actually false statements, on the one hand, and the deviation of the statement of the individual from the majority conclusion of the group to which that individual may belong, on the other. The development of the experimental method and the earlier precise pruning of the introspective method have both alike been aware for the most part of this fundamental distinction.

All sources of error can be reduced to these: partial, false, or impossible observation; imperfect enumeration; and defect in argument in developing the inferences which shall be entertained. One might go even so far as to maintain that error is to be found solely in the absolute inadequacy and inappropriateness of method itself—as has actually been the case with those positivists who find psychological methods of any and every sort inherent perversions of truth.

The genetic method does not readily fall within any one of these groups of sources of error liability. Its most conspicuous relation to error liability is to be found in the fact that it introduces a new source thereof by increasing variations which are said to demand explanation, and in thereby multiplying probabilities. Tracing analogies and making minute comparisons between remote facts within the realm of nature spread the range of error enormously. It is not easy, however, to put one's finger upon the dependency of error upon this method, since its scope and practice lack so much uniformity. In its widest genius, however, as an effort to consistently regard the soul as a rehabilitation of the psychological foundations of its ancestry, we find its liability to error to be due to the extreme infinity of variations in that ancestry and in the almost impossible task of picking out the fundamentals, for whose variations in the individual the champion of the method is to search. The genetic method has no specific right to affirm that the ancestry of your soul and the ancestry of my soul have

identical lineaments which are open to microscopic examination by the method. The aggregation of rudiments in a modern soul cannot—at least on the basis of our present knowledge of the biological relations which hedge in the genesis of a soul—be said to follow any 'laws, without introducing greater liability to error.

Another source of error in the application of the genetic method is to be found in its characteristic intention to be more interested in a constructed order of facts than in the validity of the judgment which may be pronounced in the effort to 'explain' these facts. Behind this cultivated interest lies the profound, and as yet wholly unexplored assumption that these facts become self-explanatory when arranged in the genetic order, as already pointed out.

Another tendency to error in the adoption of the method is due to the characteristic fact that the method may be regarded as wholly artificial. Why the necessity for a *re*arrangement of soul-facts in the order which is not displayed by a descriptive history of the individual soul, is not made plain by any ordinary and readily acceptable postulate upon which the whole science may rest. Here we have ample reason to believe that the 'natural' order is decidedly not the 'explanatory' order. This statement, of course, throws us back upon the recurring question, Which of the two orders is prior? It practically amounts to a confession that consciousness is so poor in its resources that it is unable to tell its own story! And of all sad things of psychological tongue or pen, these words expressing despair of method are the saddest.

In close connection with the foregoing, one should not forget to mention both the possibility and actuality of the *misuse* of the genetic method. Its partial fitness for dealing with certain data disposes some users of it to insist that it alone is fit to handle all data. This misuse may be found among those who regard the ideal of method as a necessity for offering all sorts of fanciful and absurd constructions upon the basis of development. Those who fancy that clearness of idea and continuity of thought are just as essential in genetic science as in analytical science, are among those who first turn the suggestiveness of the method into a principle of logical dissoluteness. On the other hand, a method which is constantly subject to being misunderstood is one which should receive the most careful attention and criticism from those who insist upon regarding it as the final psychological judge.

In concluding this recognition of a method which can be made to stand out so completely as a distinct form of approach to the prob-

lems of the soul, and in recognizing a method which has well-nigh swept the psychological field from one end to the other, it would be ungrateful, to say the least, not to record also some of the good features which cling to this method. All psychological facts are given in time, and genesis depends upon this condition. The method enlarges the scope of psychology, admitting many more minds, and thus many more facts along particular problems. It is a distinct gain to be instructed that genetic facts abound in the animal series. Every state of consciousness has a 'morphological' history. The genetic method stoutly opposes—superficially at least—the principle of the conservation and correlation of energy (and one might smile happily when there is found some point which forbids the physicist entering the field of psychology—as physicist, that is). The method is constructive, and brings the individual mind into relations, derived from terms peculiar to itself, with other minds, as the race mind, and especially the mind in animals. Here are vast fields brought into manageable compass by this one method of world-comparison.

In attempting its vast generalizations about the soul the method does not outstrip the corresponding rights inherent in any of the opposite methods of the science. Undoubtedly it is worth something to our science if we can definitely relate the soul to the cosmic order; but we should not wink at the fact that even the more limited and empirical character of tellurian psychologizing always keeps us on *the outside* of the soul. Taken in its very best intentions, the method enlarges the scope of, and the need for, activity within the science by raising an indefinite number of new problems, or rather by bringing every department of psychological questioning under the necessity of working over its material again in the interests of genesis. Any point of view which can do this is surely of peculiar inherent value to science. The focus of the genetic method attempts to be human consciousness. The method attempts to mediate between the extremes of the deductive and the inductive methods. We all know how difficult real induction, as resting upon experience alone, is; and as well the inadequacy of deduction, which always makes use of the opportunity of constructing its own metaphysical assumptions to suit itself. This mediation attempts to bring the soul under the larger conception of 'life,' which is alleged to be the highest, and hence, the largest, concept capable for the scientific mind. Its bondage to biology and to the postulates of biology is herein depicted in a rather deplorable fashion. One of the very excellent features of the genetic method is its generous attempt

to bring about an equivalence of soul facts for the aims of science. The gesture of the Indian, the prying of the monkey, the 'scent' of the dog, the movements of the amœba, are ranked with the analysis of the introspectionist. But the method fails us in telling definitely where 'soul' facts begin, and where they end. It nowhere really develops psychological criteria, other than the vague generality of 'life' processes.

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PSYCHOLOGICAL LITERATURE.

THE SIGNIFICANCE OF MIND IN EVOLUTION.[1]

1. *Is Evolution Progressive?*—Most psychologists to-day believe in the evolution of mind, but the character of that evolution in detail and the stages through which consciousness has passed in the building up of mental structure are subjects which are still under debate. Hence, any attempt such as this of Mr. Hobhouse to add to the slowly-accumulating mass of reliable facts concerning animal consciousness and to interpret those facts in the light of the evolutionary hypothesis is worthy of careful study by the comparative psychologist.

The question concerning evolution, Is evolution continuous? which has agitated reflective thought for twenty-five years, has gradually been giving place to the question, Is evolution progressive? It is beginning to be recognized that, just as the continuity of evolution is not a dead mechanical uniformity but a vital reconstruction with what on the surface appear as gaps and leaps and zigzags, so that same evolution is not an unbroken upward course but a spiral movement of organization of structure and function in which the process ever and anon returns upon itself in an apparently retrogressive movement. A purely progressive evolution is a philosophic myth. All anagenesis must be read in the light of catagenesis. There is nothing in a mechanical conception of evolution to ensure that it shall be progressive.

What is the source of the ideas of progress connected with the conception of evolution? Do they arise from the inevitable tendency to read evolution backwards, to interpret the process in terms of the results, just as the mechanical and materialistic conception of evolution arose from interpreting the same process in terms of its inchoate beginnings? Is either interpretation a justifiable one taken by itself? The materialistic or mechanical evolutionist has been silenced. How about the idealistic or teleological evolutionist?

We cling with the greatest tenacity to the idea of progress. What could evolution mean for us, we ask, if it were not progressive! Yet consider the facts. "If the struggle for existence has produced the wisdom of man, it has also sharpened the tiger's claw and poisoned

[1] A critical review of 'Mind in Evolution,' by L. T. Hobhouse. London, Macmillan & Co., 1901. Pp. 406.

the cobra's fang" (p. 1). "In some parts of Africa the horse is driven out by the Tsetse fly; in others the white man succumbs before the malaria germ, just as in this country the honest investor goes down before the swindling company promoter" (p. 2). "The tendency of evolution as a whole is not to produce the highest type, but rather to produce as many types as possible" (p. 4). Can we believe in progression in the light of such facts? If so, in what sense are we using the word 'progress' and what factors in the evolutionary process are its guarantee?

2. *The Place of Reason in Evolution.*—This is the problem with which the first chapter opens. This chapter is entitled 'Mind as a Factor in Evolution.' The rest of the volume may be regarded as an elaborate attempt, in answer to the implied question, to show the significance of mind in evolution, and the dominant note in the answer is that the grounds for our belief in progressive evolution are to be found in the peculiar place which reason occupies as the consummation of the whole, becoming itself the guide in self-conscious development.

The generic function of mind in organic life and evolution is "the adjustment of action to the ends of the individual or of the species, based upon a correlation of past experience, present circumstances, and future possibilities" (p. 10). The vital essence of mind is the "bringing things together so that they have a bearing upon one another. Where there is mind there is order and system, correlation and proportion, a harmonizing of forces, and an interconnection of parts. The organism which is gifted with intelligence shows it by arranging its action on a certain plan. It adapts means to ends" (p. 6). Organized action is intelligent action. Unorganized action is unintelligent. Where there is no mind at work action is random, blind, isolated, conflicting. What is done is done on the impulse of the moment and not as a means to an end. Where minds differ is in organizing power —in scope or comprehensiveness of plan, and in method of construction or execution of the plan.

"The whole process of orthogenic evolution consists in the gradual replacement of instinct by reason, and it is the final goal of reason to do precisely what is ascribed to instinct—to bring all the experience of the race to bear in organizing the whole life of the race" (but in the case of reason to do this consciously, instead of unconsciously as in the case of instinct, p. 9).

3. *General Scope of the Work.*—Before speaking of certain special topics more in detail it will be well to take a bird's-eye view of the

whole book. 'Organic Adaptability,' 'Reflex Action,' and 'Instinct' are examined in turn (Chapters II.–IV.), with the result of finding a gradual increase in complexity and variability in the method of solution of the various problems of organic adjustment. As we pass upward in the animal scale we find that pure instinct gradually gives place to a modified and generalized form which, perhaps, is best called impulse. Out of this transformed instinct, by a process of 'Assimilation and Readjustment' (Chapter V.), is developed a stage of intelligence which is the first remove from instinct. This Mr. Hobhouse calls 'Concrete Experience.' It yields what he calls the 'Practical Judgment' (Chapter VI.). From his own experimental results (cats, dogs, monkeys, an elephant, and an otter) and from his study of the experiments of other investigators, the author is led (Chapters VII.–IX.) to the conclusion that animals share with man in the possession of 'Concrete Experience and the Practical Judgment.' The principal reason for the human ascendency is found (Chapters X.–XIV.) to lie in the possession of language and social organization, which serve to facilitate the development of 'Articulate Ideas' (Chapter X.) and thus to make possible those 'Products of Conceptual Thought' (Chapter XIII.) which are peculiar to man. Finally, in three instructive chapters (XV.–XVII.) the main steps in the evolution of intelligence are schematized (Chapter XV. on 'The Stages of Correlation') and the place of 'Self-conscious Development' (Chapter XVII.) is shown in relation to natural selection. A few of the more important points will now be discussed in more detail.

4. *What is an Organism?*—According to Mr. Hobhouse, "By an organic whole is understood one which (1) has a certain general character or individuality, while (2) it consists of distinguishable parts each with a certain character of its own, but (3) such that they cannot exist unmodified apart from the whole, while the character of the whole is similarly dependent upon them. * * * It may indeed be doubted whether a purely mechanical whole exists in *rerum natura*" (p. 374). The function of the organism 'may be to evolve carbonic acid, or it may be to produce poetry' (p. 12).

An organism is only a complicated machine. "Put a penny on a balance, and it weighs down the scale by the simple action of the lever. Put it into the slot of an automatic machine, and it produces a stick of chocolate by I know not what complication of levers and cogs. Complexity of adjustment does not take us out of the region of machinery" (p. 31).

In spite of such statements, the author still seems to feel that

there is some degradation in the idea of the organism being even a complicated machine, and goes on to show what he conceives to be the difference between an organism and a machine. The chief difference he finds in the power of recuperation and self-reproduction of the organism. This he seems to regard as constituting a qualitative difference between the two.

In reply to this we would raise the question as to whether the difference between the organism and the machine in respect to this matter of self-reproduction is not simply a difference in degree. It is clear that "the continued motion of the pendulum depends upon an outside force. It has no power of accumulating afresh the energy which it loses at each swing" (p. 19). But the organism is self-maintaining only within certain limits, *e. g.*, between birth and death. It is true that, "within tolerably wide limits, it can survive accidents which cause a considerable departure from its normal course, and either struggle back to the typical life of the species again, or effect some compromise with circumstances by which life is maintained in some more or less modified form" (p. 19). But it is only a prolonged life and a relative equilibrium, since both yield ultimately (in old age and death) to 'outside' forces. No organism, unless it be, perchance, the totality of the universe itself, is really a self-maintaining and self-sustaining whole. Mr. Hobhouse's own illustrations of the compensating pendulum and the linotype might be used to prove just the opposite thesis.

The same general remarks apply to the other characteristic of the organism which is taken as differentiating it from the machine—its circular or spiral process of waste and repair. Illustrations in the organism are respiration, the vasomotor mechanism for maintaining an even bodily temperature, and the circuit of hunger, eating, strength, exercise, hunger, etc. But "a machine can be made to regulate its own action within certain limits. Thus, in the steam-engine, the forward thrust of the piston opens and shuts valves by which the backward thrust is at once brought about. Here there is perhaps a parallel to the automatic rhythm of breathing or of the heart's beat. * * * By the device of the 'governor' a steam-engine can regulate its own available energy in accordance with the work required of it. There is a close analogy here to the labored breathing of hard exercise where more oxygen is required" (p. 15).

Nothing could be better than the author's statement as to the nature of the growth process which, it seems to the present writer, is much more in harmony with the criticism just made than with the

point of view defended in the text. There is "a certain equilibrium point, as we may call it, which the organism is always striving to maintain." "The equilibrium is a moving equilibrium." "The process of rapid growth in youth, the slow change during maturity, and the gradual decay ending in death * * * is the normal orbit of every organism. The equilibrium point moves along this orbit, and the momentary changes of growth are so many oscillations about the equilibrium point as it moves." "Every deviation from the equilibrium point," within certain limits, "sets up a tendency to return to it" (p. 13).

5. *Instinct and Impulse.*—Mr. Hobhouse says (p. 25) that he follows Preyer and Verworn in defining impulsive actions as actions which "are caused without previous peripheral excitement exclusively by the nutritive and other organic processes that go on in the motor centers of the lowest rank." "An impulsive movement is due to purely internal changes." Evidently he does not identify impulse with what he elsewhere calls 'internal disposition' (Stimmung, craving, p. 57; cf. 69, 70, 95), yet this is just what such writers as Marshall, Baldwin and Dewey seem to mean by impulse. According to Mr. Marshall an impulse results from the inhibition of an instinct. Professor Baldwin calls an impulse a 'snubbed instinct.' Professor Dewey calls an impulse an 'unravelled instinct.' The suggestion is that impulse represents the disintegration of instinct and carries with it a certain amount of consciousness. On this view, this 'internal disposition' of Mr. Hobhouse would be brought out only when the instinct is thwarted or interfered with in some way. Certainly, in all that he says on the subject of ethical impulse (pp. 314, 337, 354–355) and in much of what he says under the head of the method of 'perceptual learning' (Chapter VIII.) he leans toward this view of impulse.

This, however, is simply a matter of terminology apart from certain other considerations which are involved. The suggestion here made is that much light would be thrown on these other issues by a recognition of the true relation of impulse to instinct. This brings up the whole question of the emergence of consciousness within the life of purely instinctive action.

6. *Impulse and Consciousness.*—The most important of these other questions which are here involved is connected with the relation of instinct and impulse to consciousness. Mr. Hobhouse says distinctly that he will not raise this question (pp. 79–80) and then goes on to say that 'hereditary structure' supplies sensations to consciousness,

that 'hereditary responses' yield 'sensation and feeling' to man and therefore probably to animals as well. But the problem is, under just what conditions do 'hereditary structure' and 'hereditary responses' yield consciousness? Here is just where a true psychology of impulse fills a gap in the evolution of mind. As has just been said, Mr. Hobhouse does not elaborate this point, but his statement as far as it goes is almost identical with that of Mr. Morgan on the same point. It will be profitable to refer to the statement of the latter in this connection. The criticism offered upon his view will apply to the less explicit doctrine of Mr. Hobhouse.

Mr. Morgan ('Habit and Instinct,' p. 136; cf. summary, pp. 323 f.) says that "on the occasion of the first performance of an instinctive activity the coördination involved is automatic, and cannot be regarded as under the guidance of consciousness; but that the carrying out of the activity furnishes data to consciousness in the light of which the subsequent performance of a like activity may be perfected, or modified or checked." "From this it follows," he says, "that only on the occasion of its first performance does such a congenital activity present itself for our study in its instinctive purity. For on subsequent occasions it is more or less modified by the results of the experience acquired by the individual."[1]

Now the question is, How and why does an activity which is relatively so perfect as a congenital instinct 'furnish data to consciousness'? If the reaction takes place in the first instance without consciousness, why should consciousness be developed in connection with it later? What Mr. Morgan's theory lacks is a sufficient explanation for the emergence of consciousness at this point, or for the fact that the 'carrying out of the activity' at this point or at any other point 'furnishes data to consciousness.' But on the theory which is defended by the present writer,[2] the emergence of consciousness is connected with some break in the adjustment process by which the animal is endeavoring to adapt itself in its environment. The definite congenital instinct will continue 'in its instinctive purity' as long as the process of adaptation runs smoothly. But if there is any serious friction in that process of adaptation, some new reaction is demanded, or some modification of the old one, and it is at the point of and for the sake of this new need of the organism that consciousness appears as the medium in which the new mode of response is built up.

[1] Cf. also his statement in 'Animal Behaviour' (1900), p. 332, that 'organic evolution provides ready-grouped data to consciousness.'

[2] Cf. *Journal of Comparative Neurology*, Vol. XI., No. 2, for a fuller statement.

Mr. Marshall speaks of 'instinct-feelings' as the subjective accompaniments or conscious coincidents of 'instinct-actions' ('Instinct and Reason,' p. 86), and shows how this 'consciousness coincident with the instinct-actions' arises. He shows that impulses "are mental phases which in an objective view we always find to be determined by the inhibition of instinct-actions as these are more or less modified by experience; which instinct actions have been stimulated by the presence of conditions that might normally call them out, but which for one reason or another are not at once realized" (p. 94; cf. Chapter XIII.). He says (p. 342) that every instinct "implies the possibility of the appearance of an impulse, provided the conditions of stimulation appropriate to the expression of the instinct are realized, yet under certain forms which restrict this expression." He denies Professor James' contention that 'every instinct is an impulse.' "When we see a man aim a quick blow at an enemy suddenly appearing before him we say that the actions involved express the instinct or capacity within him; but it is when we see him restrain this action under temptation that we properly say that he must have had an impulse which would have led him to strike his enemy had it not been restrained in one way or another" (p. 342). "We act instinctively in a thousand different ways during all our life without paying any attention to the acts; but some day, when something inhibits our instinct actions, then we have a disturbance of our mental life, which in complex cases produces what we designate an impulse" (p. 343). "In no case does the impulse appear in consciousness except as the result of an obstruction to the realization of certain activities which are determined by the existence within us of coördinated neural structures" (p. 344). The activities, if they find expression, which accompany such a state of consciousness are random, uncontrolled, haphazard, unmediated. Impulse is characteristically accompanied by an emotional consciousness, by what Mr. Marshall calls 'instinct-feelings.'

This, as I understand it, is the essence of the view of impulse as held by Professor Dewey and Professor Baldwin. The original fact of all experience is its character as a movement or tendency to action. Interrupted or obstructed activity gives rise to feeling. Out of feeling sensation or cognition is gradually evolved. Impulse represents the transition, the emergence of the conscious out of or, better, within the unconscious. Mr. Marshall supplies what is lacking in Mr. Morgan and Mr. Hobhouse. The bearing of this on the theory of the criterion for the presence of consciousness (Hobhouse, p. 82) and of the distribution of intelligence in the animal world (pp. 103–111) is

obvious. Curiously, there is no mention of Professor Loeb's researches.

7. *Instinct and Reason.*—Intelligence Mr. Hobhouse defines as 'the power of an organism to adapt action to requirement without the guidance of a hereditary method of adjustment' (p. 82). In the growth of this power of correlation of its own past experiences with its subsequent action lies the evolution of animal mind. But instinct and intelligence or reason, though opposed in idea, are so far from being incompatible in fact, that it is actually within the sphere of instinct that intelligence first arises (cf. 270, and especially, pp. 77–79, a remarkably good passage). Here, again, by some strange oversight, there is no mention of Mr. Marshall's almost identical view in his 'Instinct and Reason.'

First, it is pointed out in what respects the scope of animal consciousness is restricted (pp. 312, 314, 315, 320, 321). Second, it is shown how in detail reason develops within instinct (pp. 58–79). "The impulse to reason is itself an instinct" (p. 318). Third, instinct furnishes the main outlines of experience, even in the case of man who possesses reason. "Instinct lays the ground plan of conduct, within which, details may be remodelled by individual experience" (p. 320). Instinct is finally transformed and evaluated by reason while still furnishing the content of experience (pp. 357, 370–372). Fourth, the elementary form of reason consists in connecting hereditary modes of reaction (instincts) with definite objects or situations—'a form of the defining or particularizing of instinct' (pp. 107, 108). Fifth, it may roughly be said that instinct (when broken up in impulse[1]) presents the ends of experience, and reason works out the means. "The first function of intelligence is to define the proximate ends of instinct, and thereby to render experience available in the choice or revision of means" (p. 270). Finally, the chief difference between man and the lower animals is that in the lower types of consciousness the attention is concentrated on the response rather than on the stimulus in the organic circuit (cf. pp. 142, 143). The civilization of man is simply the concentration of attention on the stimulus, on a large and elaborate scale. The response is the first phase of the organic circuit to come to consciousness because it is most directly connected with action. This might have some bearing on the difference between the so-called 'sensory' and 'motor' types of subjects in reaction experiments.

8. *Perception and Conception.*—The use of the term 'perception' by the author is open to criticism. It is difficult to believe that

[1] As above outlined.

there is any ambiguity in Mr. Hobhouse's mind as to the relation of perception to conception, but certainly it is not made in this book to stand out as clearly as his use of the term 'simple apprehension' in his valuable 'Theory of Knowledge.' 'Perceptual acquisition' (learning by perception of results) is distinguished from 'motor acquisition' (learning an act by doing it), on the one hand, while it is quite carefully marked off from conceptual processes, on the other. The use of the term is a common enough one, but not the best one, for perception arises only *with* conception, whereas Mr. Hobhouse, like Romanes and others, makes it precede conception.

His use of such terms as assimilation and association and practical judgment are apparently an attempt, like the term reception of Romanes, to fill the gap between perception and conception when thus conceived as successive stages of cognition instead of as complementary aspects. One sees evidence of the influence of the English empirical psychology in certain passages (cf. pp.135, note, and 135–136, summary). Mr. Hobhouse says that 'assimilation does not necessarily involve ideas at all' (pp. 113, 114). How he is to reconcile this with his other statement on the previous page (p. 112) that all experience 'is in a sense experience of a relation,' is not made clear.

According to Mr. Hobhouse, where there is analysis of the relations implicit in a perception we get conception. But the knowledge of the situation as a whole without the relations being dissected out as distinct elements is perception (p. 117; cf. p. 124 and summary, pp. 135, 136). It certainly is an unfortunate use of the term 'perception,' because of the ambiguity. What we need is some unambiguous term to mark off the indeterminate stage of cognition from that determinate stage in which both the objects (of perception) and the relations (of conception) are distinguished within the cognized whole.

Mr. Hobhouse quotes Professor Thorndike with approval when he says that the animal, like the man swimming, 'simply feels an impulse from the sense-impression,' and implies, at least, that the animal has the *sense-impression* of the water, the sky, the birds above, as well as the impulse. But is not just this distinction the mark of the higher type of intelligence? The animal, like the man swimming or playing tennis, does not feel the sense-impression as such at all. He feels the impulse and it is only a subsequent analysis which reveals that the sense-impression was latent in this vague kinæsthetic consciousness of impulse. Perception and conception represent not successive stages but correlate phases within the cognitive function: they appear together and they vanish together.

9. *Methods of Learning.*—One of the most helpful discussions in the book is that which attempts to answer the question, How do animals learn? According to Mr. Hobhouse, the fundamental underlying principle of learning is that a wave of excitement once started in the nervous system persists for a certain short time (p. 93). Out of this grows the possibility of 'assimilation and readjustment.' This elementary correlation is the first stage beyond instinct in the evolution of mind. Its operation is confined to adjusting reactions suitably to their immediate results. If more remote correlations are to be effected, it must be by a very slow and gradual process. This renders it possible for animals to thrive without highly definite instincts and brings about the substitution of more or less general tendencies and impulses for the more narrowly defined hereditary methods of action.

It is not necessary to dwell on the unlearned type of reactions or the instinctive method of adjustment, except to say that in this connection the author calls attention to many human adjustments which are still practically on the animal level (pp. 310, 311, 318). More important is the method of trial and error or, as Professor Thorndike calls it, the method of trial and success. Mr. Hobhouse calls this the method of assimilation. All learning by experience has in it the element of rationality. Pleasure and pain play an important part in 'stamping in' a response (pp. 85 f., 98, 99, 141, 142). One trial may be enough to teach the animal, but the effect of the experience tends to wear off with time (p. 87; cf. 84). Most interesting is the treatment of what Mr. Hobhouse calls 'the critical success' (pp. 204, 267, and *passim*). Mr. Small, in his study of the rat, has called attention to the same thing. In the function of the critical success we have the first intimation of the method of learning by ideas. The real transition from the brute to the human took place when the animal first became conscious of the fact that intelligence or ideas are a valuable factor in the struggle for life as well as mere physical prowess or brute force. It is immaterial whether this discovery was made first by a biped such as the anthropoid ape or not. Whenever it was made, it placed the animal at an immense advantage over its fellows. Henceforth rational acts are not wholly of the trial and error type but involve the beginnings at least of deliberation in its twofold aspect of reflection and anticipation. Rational acts from this point on become more or less spontaneous and free instead of the result simply of necessity and coercion. "As ideas become more articulate, the results of experience are more freely combined or modified to suit practical

needs. Something like originality begins to show itself, and we have instances of what we have called 'spontaneous application'" (p. 234).

The three modes or types of reaction may be illustrated as follows. "Three persons start for a certain place. One does not know the way, but is directed to follow a certain road. Keeping to this road, he arrives safely and speedily unless there should be any unforeseen obstacle, such as a broken bridge, in which case, as he knows no other paths, he is blocked" (this is the case with the instinctive method of reaction). Another "wanders at random, but as everywhere there are hedges and walls preventing him from getting far out of the way, and as hedges grow up behind him to prevent his return, he gradually arrives by eliminating all possibilities of going anywhere else" (this is the method of trial and error). The third "knows where the point is, and finds his way there, going by a detour if the direct road is impassable" (this is the method of ideas, p. 400).

10. *The Function of the Universal in the Evolution of Intelligence.*—"We are thus brought to the primitive function that ideas fulfil in conduct. As long as impulses are fixed in relation to stimulus, whether by heredity or habit, action neither requires nor tolerates any further guide. But if the ends of an impulse are to be served by actions varying from case to case, a uniform reaction to uniform stimulus will no longer do, and the case is met in the human world by a formulation of the end to which one is impelled, along with its relation to the surrounding circumstances. The formulation of an end constitutes an idea, and the impulse so qualified becomes a desire. In other words, so long as stimulus guides action in a uniform manner, no idea of the end is required. Where the point to which action is directed must be defined specially for each action, there an idea is, in human conduct, necessary" (pp. 130, 131). This may be regarded as the particularizing function of the idea.

"The universal judgment is not so much a reference to an indefinite number of particulars as a rule of reference. * * * Its function in thought is (*a*) to sum up the result of a mass of experience, and (*b*) thereby to form a guide in dealing with a further mass of experience to come. Under both aspects it brings the action of the moment into explicit relation not merely with the immediate circumstances of the particular end, but with masses of experience past and future" (pp. 298, 299). The universal thus renders explicit 'influences which have already been operative without being expressly formulated' (*e. g.*, instincts, habits). 'When the results of experience can be rapidly

summed up and communicated' in and through the universal, action becomes more effective. This ability to build up a world of ideas or of universals by a correlation of his experiences in masses or systems is one mark of man's ascendency over the lower animals (p. 299). This may be regarded as the universalizing or generalizing function of the idea.

In the pre-reflective stage "what corresponds to the major premiss is a certain formed disposition, what corresponds to the minor a stimulus, what corresponds to the conclusion a response" (p. 323). "The premisses in this case are antecedent conditions from which the response follows, but there is no evidence that either of them is grasped in relation to the response or its results" (p. 323). In the stage of the practical thinking of the plain man and of the higher animals "the starting-point is a perceived relation — as of action and consequence — an 'observed particular,' and the result a judgment equivalent to the combination of minor premiss and conclusion, the major being still represented only by the mental habit which predisposes toward the combination" (p. 323). "In the reflective stage the major premiss itself becomes explicit and the syllogism complete. We have a universal judgment, the particular, and their combination in the conclusion. * * * The apprehension of the universal appears as a turning round of the mind upon its previous operations; a bringing into clear consciousness of what it was doing before" (p. 323). "Thought in this stage may therefore be typified in the completed syllogism with explicit major premiss as contrasted with the truncated syllogisms of the previous stages" (p. 325). Nowhere have I seen a statement which brings into clearer relief the true relation between the *idea* of psychology and the *universal* or *concept* of logic, and the relation of both to the pre-conscious modes of action. In this connection attention should be called to the valuable discussion in Chapter VI. of 'Concrete Experience and the Practical Judgment.' The practical judgment, according to Mr. Hobhouse, is intermediate between habituation on the one side and reasoning on the other, and in it he finds the key to the behavior of the higher animals as well as to many acts of human beings which are still on the animal level. Chapter XV., which is a 'Summary on the Stages of Correlation,' ought to be read in connection with the article by Professor Dewey on the same subject in the *Philosophical Review* ('Some Stages of Logical Thought,' September, 1900).

11. *Natural Selection by Conscious Adaptation.*—We may now return to the question with which we set out, Is evolution progressive? and answer that evolution along the line of rational intelligence

is progressive, because its guarantee lies within itself. All other evolution either tends to be stationary or tends toward retrogression. "The truth is that organization as a method of maintaining the species is set from the first in antithesis to natural selection. Natural selection rests on destruction. It maintains the type only by sacrificing the majority of individuals" (p. 387). "Natural selection can preserve and augment nothing that is not immediately useful. If the pigment-fleck which is the first rudimentary germ of the eye is preserved and developed, it must be because it is useful as a pigment-fleck. The plea that it will later develop into a magnificent sense-organ of the highest possible utility could not avail it in the court of natural selection unless it could prove services actually rendered by itself" (p. 391).

But with intelligence comes a latitude and development in scope as well as refinement of organization, and this latitude is a necessary condition of the highest development. The advent of intelligence means a revolution both in the method and in the rate of progress in evolution (cf. pp. 382, 383, 401–403). "Organization, especially in the form of intelligence, sets rather to maintain the individuals, and in so doing improves the type. The rational organization of life, from the dawn of parental care upwards, tends to suspend the struggle upon which natural selection rests * * * culminating in the deliberate self-development of a race under the guidance of reason. Organized life rests not on internecine rivalry, but on mutual interdependence" (p. 388).

Natural selection means limited supply of food, overproduction of individuals, struggle to the death of these individuals for this food, and either the elimination or the involuntary mutual adjustment of the individuals. Self-conscious evolution means controlling the supply of food and the environment in general, controlling the production and education of individuals, and the conscious coöperation of the individuals in mutual adjustment. This is just as *natural* a process of evolution as so-called 'natural' selection. The anti-social unethical individual goes to the wall here just as the weakling goes to the wall in the struggle for life. But the 'struggle' here is to make the unethical individual ethical, to make him worthy to survive. The category of worth and right transforms the category of chance and might, and the survival of the fittest is changed from the heartless competition of the ruthless struggle for life into an ethical coöperation of those who have survived to the end of fitting as many as possible to become worthy to survive. Pains and pleasures are substituted for death and life as the sanctions of conduct (p. 388), and, under the guidance of intelligence,

it is possible and even probable that "among the pleasures on which experience lights should be some connected not with the maintenance of the race at its then level, but with the further expansion of its powers. Such an expansion may be of little use to it as a means of survival for the present, but it means progress hereafter. Something like this would seem to be the history of those mathematical and æsthetic 'faculties' which have been a stumbling-block to natural selection" (p. 390).

But natural selection and conscious adaptation are not two antagonistic or incompatible processes. There is evidence that these coöperate in many instances for the preservation of a structure and its function. Thus, to use Mr. Hobhouse's own illustration, the contact of the hairs or the odor of the meat leads the insect to deposit its eggs, and this is a conscious reaction. But these insects attach themselves with equal readiness to any other hairy surface (besides the body of the bee) or deposit their eggs in the flowers of the carrion plant, the smell of which resembles that of putrid meat, and in this way many perish. The so-called law of chance and the principle of natural selection obviously enter here. Thus the actual survival of the type depends upon the concurrent operation of blind natural selection *and* a simple mode of conscious adaptation (cf. pp. 49 and 52).

Just because consciousness follows the center or area of tension or effort in the adaptation, the great mass of intelligent reactions has become automatic: these are preserved as unconscious reactions by heredity. "The smell of putrid meat attracts the gravid carrion fly. That is, it sets up motions of the wings which bring the fly to it, and the fly having arrived, the smell and the contact combined stimulate the functions of oviposition. The sight of appropriate food stimulates the chick to peck, just as the contact of the food with the interior of the bill stimulates the swallowing reflexes. And just as the sight of the food stimulates the chick to peck, so the sight of the chick stimulates the hen to cluck, or to scratch for food, or to protect it from danger, and so forth" (p. 53).

Now this may take place all unconsciously, automatically, or its salient feature may be conscious and voluntary. It may be all a mere matter of 'response of inherited structure to stimulus,' and again it may not. That depends upon circumstances. The whole chain of reflexes involved in any such instinctive act would not, of course, be in consciousness. The greater part of any organic circuit has been mechanized and relegated to the unconscious background, the subliminal life. But the salient feature, the prominent or crucial aspect,

will remain in the focus of attention or, if partially mechanized, will be brought back into the focus of consciousness because of the organic tension at that point. If the animal encounters no difficulty in finding the proper place to deposit its eggs or to find its food, these processes might go on in a purely reflex way. But if there is opposition to be encountered and if a search is necessary and if there is danger to be avoided, then it is most natural to suppose that there is a corresponding consciousness which serves both as a monitor and as a guide, not taking the place of instinct or reflex action, but directing it to finer issues or adapting it to varying circumstances. As Mr. Hobhouse himself remarks, it does not follow because an animal performs a given series of acts mechanically on one occasion, that that animal is destitute of consciousness, or that it is not able under *any* circumstances to bring consciousness to bear on the action. Under other and perhaps greater conditions of tension the situation might call forth a conscious reaction. "Many human actions are performed mechanically day by day, but a sufficiently strong stimulus directs attention to them, and brings intelligence to bear" (p. 55).

12. *The Principle of Projected Efficiency.*—Mr. Hobhouse's general conclusion is so similar both in spirit and in mode of statement to that of Mr. Kidd in his recent book on 'Principles of Western Civilization' (Macmillan, N. Y., 1902, pp. 538) that a brief comparison of the two may be instructive. Mr. Kidd writes, "The winning peoples who now inherit the world are they whose history in the past has been the theater of the operation of principles the meaning of which must have at every point transcended the meaning of the interests of those who at any time comprised the existing members of society" (p. 5). "The controlling center of the evolutionary process in our social history is, in short, not in the present at all, but in the future" (p. 6). This Mr. Kidd calls the 'principle of projected efficiency.' It follows that "the determining and controlling end towards which natural selection has been operating, must have been, not simply the benefit of the individual, nor even of his contemporaries, in a mere struggle for existence in the present, but a larger advantage, probably always far in the future, to which the individual and the present alike are subordinated" (p. 50).

Compare with this the words of Mr. Hobhouse. "We can conceive as not indefinitely remote a stage of knowledge in which the human species should come to understand its own development, its history, conditions, and possibilities, and on the basis of such an understanding should direct its own future" (p. 336). "Remote as this

ideal organization of life may be, it is suggested that the trend of theoretical science is towards the discovery of the conditions of human development, while the trend of the ethical spirit is towards making development the supreme object of action. In the union of these movements, human thought would seem to come as near as possible to the limiting conception of the correlation of all experience with all action. At any rate, knowledge of the underlying conditions of development would become the basis of a system of conduct designed to promote development. The life of the species would become self-conscious, and its growth self-determined" (p. 357). "We start with a consciousness limited to the reaction of the moment, and knowing nothing of the past which determines its action, nor of the future which its action will affect. Step by step, as we advance, more of the past and the future come within the scope of intelligence, and we end at a point where all that has made the race what it is is brought into the account and made to prove what it has in it to be. At this stage the mind of man is first fully self-conscious in the strict sense—conscious of its own nature, of the conditions under which it lives and works, of the future to which it may aspire" (pp. 336, 337).

It is stimulating to find in the sequel that the evolutionary theory has really been taken seriously and not, as is the case in so many books which profess to build on the doctrine of evolution, as a mere architectural convenience in the construction of an absolute system on a fixed preconceived foundation. The following quotation may fitly close this review, for it sums up what, in the present writer's opinion, is an insight into the true philosophy of evolution:

"Now it is easy to show that in such a system the ultimate ground of interconnection can neither be purely mechanical nor purely teleological. Reality is or includes a time process. Now, if we take any time process, and consider its beginning, we are dealing with a partial fact, and for every partial fact, thought demands an explanation which will connect it with reality as a whole. For the cause of the origin of a process, then, we may look in two directions, to its results or to its antecedents. If we look to the latter, we are clearly going outside the process. But if the process is one in which the whole nature of our ultimate system is to be expressed, we cannot go outside it without denying the claim of our system to be complete. We are therefore thrown forwards towards the results of this system. But neither can the purpose achieved by the process stand alone, for the necessity of the process must also be made plain. If an unconditional purpose were the secret of the universe, there could be no

explanation of the means, the process, and the effort through which the purpose is realized. From the conception of purpose, then, we are again thrown back on origins, just as these throw us forward to their purpose. We have, in short, to conceive a single principle not realized in full in any one phase, but pervading the whole world-process. In this principle, the possible and the actual in a sense come together, for what it is to be is an integral condition that goes to make the world what it is. We cannot take any phase of reality as an absolute starting-point and regard it as determining everything that follows upon it mechanically, or everything that precedes it teleologically. If we conceive any process as making up the life of an intelligible world-whole, we must conceive its origin and issue as dependent on and implying one another. That is, we must conceive it as determined organically" (pp. 404, 405).

H. HEATH BAWDEN.

VASSAR COLLEGE.

Psychology, Normal and Morbid. By CHARLES A. MERCIER. London, Swan Sonnenschein & Co.; New York, The Macmillan Co., 1901. Pp. 518.

There are few fields of psychology that require illumination more urgently than the relations between the normal and the abnormal; there are few topics in psychology concerning which a readable volume would receive a more cordial welcome than the topic which Dr. Mercier's title suggests. One of the phrases that the author lets fall, apropos of a very different application, is that 'the practice in psychology is that anyone may call anything by any name that he pleases.' The restrictions opposed to the indulgence of this propensity in psychology, are perhaps less stringent, yet not different in type, from those obtaining in other disciplines. Yet Dr. Mercier has used—I had almost said abused—this privilege in the correlation of content and title in the present volume.

It is always a delicate, sometimes an impertinent, task to find fault with an author for not doing that which he never intended to do. The 'pursuit of happiness' clause when applied to authorship seems to include the privilege of planning one's own tour, choosing one's own vehicle and route, taking one's own pace, and letting those follow who will. None the less from the point of view of serviceability, either as the record of an individual journey, or more directly as a guide-book to other tourists, the consideration of the probable interest, capacities, general information and equipment of the travelling public, will not

only in the long run, but in a comparatively short run, determine the longevity of any such contribution as Dr. Mercier has prepared.

It is rather easier to describe what the volume does not accomplish than what it does; the needs which it fails to meet, than the interests to which it will specially appeal. Lloyd Morgan's 'Introduction to Comparative Psychology' furnishes a possible model for a similar treatise upon abnormal psychology. The plan pursued is the presentation of those aspects of mental processes in general which find their most direct applications in the special field (comparative or abnormal psychology) concerned; this in turn followed by a consideration, in the light of principles thus established in normal psychology, of the special problems of the genetic or disordered mental processes. Such a volume has the possibility of serving as an introduction for students of normal psychology to the field of the abnormal, and of supplying those whose main interests are in the abnormal with a suitable foundation for their superstructure. Maudsley attempted such a service many years ago and (considering the psychological needs and interests of the day and his own purposes) with distinct success; the recent book of Dr. Störring aims to meet a similar need. No one, to my knowledge, has pursued so systematic a plan as Lloyd Morgan, or has performed so serviceable a function for abnormal psychology as he has done for comparative psychology. The hope of such service aroused by Dr. Mercier's title and by the ability shown in his previous contributions, is not justified. This field remains free for further effort.

Looking aside, however, from this purpose—a purpose, which Dr. Mercier's preface would indicate that he really contemplated—there are many things to be said in favor of the interest and value of what the volume contains. Not the least of these is that the book contains few dull pages; it is thoroughly readable. The style is clear, the illustrations apt, the central purpose in the several discussions well maintained, the positions stated with originality, and defended with ingenuity. It is a volume that the psychologist will assign to an accessible place upon his shelves.

The most striking feature of the group of topics represented is the prominence of the discussion of logical principles. Nearly half of the extensive volume treats of matter usually assigned to books on logic. Dr. Mercier does this with distinct intent, holding that the study of thinking is essential to an understanding of right and wrong, normal and morbid thinking. That part of the discussion that is distinctly aimed at the psychological analysis of belief-formation is,

indeed, pertinent; and the section upon delusive beliefs is one of the best in the volume. But in spite of a deep interest in the logical discussions, I cannot convince myself that more than one third of the discussion there introduced has direct pertinence to the main theme, nor that the general psychological reader will so far tolerate these discussions as to read them, or reading them, profit much by them. Students of logic will do well to consult some of Dr. Mercier's discussion; they, however, are not likely to look for them in a volume on abnormal psychology.

Apart from the consideration of the nature and validity of the thought-processes, both inductive and deductive, we find a presentation of the function of sensation, of memory, of volition, of pleasure and pain, and of consciousness, in the mental life; and in each case, though in a subsidiary and less extensive manner, some consideration of the abnormal aspects of these processes and their contribution to the symptoms of mental derangement. The interest and point of view throughout is that of the analytic student of normal mental states, utilizing the data of mental disease. No systematic account of the types or forms of occurrence of mental abnormalities will be found; in other words, the discussion is addressed to the professional student of normal psychology; the alienist seeking aid and enlightenment on matters psychological is not likely to be encouraged if his interests happen not to be those of Dr. Mercier.

In fine, the volume represents an able, suggestive, original, clear and helpful discussion of a group of main problems of the psychological life. The tone and temper and content of the discussion are the individual expression of the nature of Dr. Mercier's interests. It is a personal contribution and as such must in the end be valued. As a guide-book it seems destined to furnish readability combined with disappointment. As a record of what one traveller had eyes to see, and a ready pen to describe, it will have considerable value to others intending to travel, in this part or in that, the same route. This individual interest has led Dr. Mercier to adopt the methods of a pioneer in regions in which very considerable clearing of the ground had already been done. His utilization of the works of others is inadequate, leading him in lesser instances to profess originality in matters which others had previously set forth.

My chief contention is my chief regret—that Dr. Mercier did not decide to write a text more systematic, more consistent with his title, more clearly adapted to the services now most urgent in the field of abnormal psychology. As a series of chapters contributory to a clearer

analysis and portrayal of a group of psychological processes, the work promises to acquire decided value. It stands as Dr. Mercier's most important, as it is his most comprehensive, contribution to psychology.

JOSEPH JASTROW.

UNIVERSITY OF WISCONSIN.

VISION.

A Method of Mapping Retinal Circulation by Projection. R. M. OGDEN. American Journal of Psychology, Vol. XII., pp. 281–291.

Mr. Ogden's method consisted in having his subjects fixate on a bright cross-lined screen, and observe the course of those fleeting bright points which are evidently connected in some way with the retinal circulation. The subjects carefully noted on similarly cross-lined paper the form and directions of these movements; and as a check and supplement, also observed and recorded the projection of the blood vessels produced by light playing upon the retina through a small moving aperture before the pupil. Two sample maps obtained in this way accompany the article, and from these it would appear that the courses of the moving points often exactly coincide with the shadow-projection on the blood vessels, thus adding to the evidence that the darting points are due to the flow of the blood. It also appears from the maps that the observation of the moving points gives courses that do not appear at all in the projection of the blood vessels themselves; although it is also true that in numerous instances blood vessels appear as shadows, of which there is no indication from the darting points. A farther advantage of these maps over those obtained solely by vascular projection is that they give the direction of the flow of blood in those vessels where the bright movements are noticeable.

The author believes that the evidence favors the view (modified from Helmholtz) that the more definite bright moving forms are due to 'chance spaces between corpuscles or bundles of corpuscles in the normal flux' of the blood. That they are not the projection of the moving blood corpuscles themselves seems probable from the fact that the spots are brighter even than a bright field. If there were to be any projection from them at all, it ought to be darker, since the shape of the corpuscles is not such as to concentrate the light, as Boisser held. The paper includes a good historical account of the method of observing retinal circulation, as well as of the theories to account for the darting points in the visual field.

GEORGE M. STRATTON.

UNIVERSITY OF CALIFORNIA.

Uber stereoskopische Lupen und Brillen. EMIL BERGER. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. XXV., pp. 59–77.

Die normale Refraction des menschlichen Auges. M. STRAUB. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. XXV., pp. 78–100.

These two articles are primarily for the oculist and optician, but are not without interest for psychologists as well. Dr. Berger describes an improvement he has made in the *loup*—the eye-glass used by watchmakers, engravers, and others employed in very fine work. In spite of the many advantages of binocular instruments, no binocular form of the loup has hitherto found favor, because the field of view in binocular instruments is much reduced and the magnifying power is usually too great. The author, starting from the form of glasses used by Brücke and by Liebreich (where short-focus convex lenses for each eye are placed so that their centers are closer together than the interocular distance) reports that he finds a great gain by simply inclining the glasses toward each other, not more than 15°. The nasal portion of each lens now inclines inward and backward, the temporal portion outward and forward. The result is a much larger field, all strain of accommodation and convergence, it is said, is done away with, and there is a heightened binocular depth-effect. Unevennesses of an observed surface amounting to $\frac{1}{50}$–$\frac{1}{100}$ of a millimeter are found to be noticeable.

Dr. Berger lays great weight on this horizontal inclination of decentered lenses. A certain amount of astigmatism results from this arrangement, but it is equal and opposite to the normal astigmatism of the eye; it is consequently no disadvantage, but rather a correction. The author has also devised a modified form of his loup which he calls stereoscopic 'spectacles'; these have an advantage for certain kinds of fine work. He even recommends in many cases the use of inclined glasses for ordinary reading, to avoid the disturbances of coordination of the two eyes which the usual glasses often produce. But for these details, well presented with diagrams, the original paper must be consulted.

For the psychologist, one of the most important points of the paper is the evidence brought forward that stereoscopic vision can improve or deteriorate according to practice. Many persons whose work is monocular—the Swiss watchmakers, for instance—find it of advantage to drill themselves, outside of working hours, in binocular vision. The neglect of the field of one eye while working with the

monocular loup tends to the neglect of that field even when the loup is not worn. The psychological suppression of the image in one eye often causes this eye to cease its suitable muscular coördination with its fellow; strabismus results, and the person becomes incurably monocular. The use of the binocular loup, Dr. Berger reports, has in his own case helped him to make good the loss he had suffered through the use of the monocular microscope. Several watchmakers reported good results also from the use of the ordinary stereoscope, as a corrective of the results of their prolonged monocular work.

Dr. Berger's use of the word 'stereoscopic' in connection with his own instrument should not mislead. He himself points out that its optical principle is entirely different from that of the stereoscope. His motive for introducing the word seems to be to give emphasis to the binocular depth with which his instrument affords, and even exaggerates, when one views solid objects through it.

The central thesis of Professor Straub's article is that the normal human eye, when accommodation is artificially annulled, does not focus distant objects on the retina, as has usually been held. Instead of being emmetropic, the normal eye is anatomically hypermetropic, and is made emmetropic only by the tonic tension of the ciliary muscles. The normal anatomical under-refraction, which is physiologically offset in this way, amounts to 1–1.5 dioptres.

This underlying anatomical hypermetropy appears even in a larger measure at birth, when it amounts on the average to about 2 dioptres, and with wider variations than in later life. After birth it gradually diminishes, to increase again with old age. Only the more decided hypermetropy, coming usually after 60 years of age, is to be attributed, with Donders, to a change in the refractive power of the lens. The milder forms of senile under-refraction the author believes (as against Donders) are due to the loss of ciliary muscular tone, and a consequent return to the normal anatomical refraction of the eye. The variations of normal refraction are thus in a large measure physiological; only at the beginning and end of life is there much of an alteration of the passive shape of the eye.

Of especial interest, sociologically, is the fact, brought out by an examination of the schools of Amsterdam, that when the parents are higher up the social scale, the children's eyes become emmetropic much sooner than for the lower classes socially. And this, apparently, quite apart from any inducements to earlier study. It is also interesting that in the individual the tendency toward emmetropy is so strong that when the anatomical form of the eye is emmetropic and

the natural tonus of the ciliary muscles would consequently produce myopy, the natural tonic contraction disappears.

GEORGE M. STRATTON.

UNIVERSITY OF CALIFORNIA.

Ueber Bewegungsnachbilder. A. BORSCHKE und L. HESCHELES. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Bd. 27. Pp. 387–402.

This is a report of experiments conducted in the Physiological Institute of the University of Vienna with a view to determining the rate of apparent movement in after-images resulting from the observation of moving objects. Two sets of rods—one upright with a horizontal movement, the other horizontal with a vertical movement—were observed through a circular opening in a screen. One set remained constant in regard to speed, number of rods, degree of illumination and time of exposure; the other varied. Two after-images were produced, each moving in a direction opposite to that of the real movement. These combined in a resultant image, the direction of which served as a basis to measure the speed of the variable component.

The experiments showed that the speed of the after-image is, within certain limits, directly proportional to the speed of the original movement; that it increases with the number of impressions received per second; and that it is affected by the brightness of the stimuli and by the length of the observation. The authors, however, call attention to the fact that their method of measuring the speed is valid for a few seconds only in each observation. Longer periods showed considerable fluctuation in the direction of the after-image and interfered with the determination of its speed. E. A. PACE.

CATHOLIC UNIVERSITY, WASHINGTON, D. C.

RHYTHM AND TIME.

The number of the *American Journal of Psychology* for January, 1902, contains three papers on time and rhythm which may well be reviewed together. The articles are as follows:

The Relation of the Fluctuations of Judgments in the Estimation of Time Intervals to Vaso-Motor Waves. Study from the Psychological Laboratory of the University of Michigan. H. C. STEVENS. Amer. Jour. of Psych., XIII., 1–27, January, 1902

Accuracy in the judgment of time intervals is shown by Mr. Stevens to apparently correlate with blood supply. The periods when

judgments are least accurate seem to coincide with a rise in the vaso-motor wave as recorded by a finger plethysmograph. There is thus demonstrated a tendency of the blood to flow away from the brain when this higher mental activity is at its ebb.

The numerous experimental difficulties in securing satisfactory records of the dilatation of blood vessels simultaneously with psychic events gives this work of Mr. Stevens considerable value. The article contains several plates in which a direct comparison may be made between the fluctuation of time judgment and the course of the vaso-motor waves. This method, however, Mr. Stevens admits is not so convincing as that of averaging the errors in judgment which are made at the crest of the vaso-motor waves and comparing them with errors made coincident with the trough of the waves. A table of averages for 321 judgments made on intervals varying from .18 to 2.4 seconds indicates greater errors at the crest of the waves. The table might have been improved by giving the variability of the averages.

A second part of the study deals with the amount of the errors in time estimation and notes their failure to follow Weber's law. So far as constant errors are concerned he finds intervals below .72 sec. over-estimated. From that to 2.4 sec. underestimated. From 3.7 to 7.24 sec. overestimated. These tendencies agree with Mehner's observations.

Respiration was also recorded during the experiments, but Mr. Stevens concludes that it played no significant part in the judgments except for intervals of 3.7 sec. or longer, when he finds the termini of the intervals occurring at similar points on the breathing curves.

A Contribution to the Psychology of Rhythm. By CHARLES H. SEARS, Fellow in Psychology, Clark University. Am. Jour. of Psych., XIII., 28–61, January, 1902.

The article deals with the time element in instrumental music, and follows one contributed by Mr. Sears last year on a different phase of rhythm. For this investigation four musicians played on an organ parts of four well-known hymns. The keys of the organ were electrically connected with pens writing on a kymograph. The time record of the soprano part was thus recorded. An interesting feature of the paper is the electrotypes of the music scores giving below each note its value in hundredths of a second for each player. Tables are also given showing the average length of measures, of whole, half, quarter, eighth and dotted notes. A similar analysis is made of the

time relations of two selections played by a music box. The investigation follows somewhat the work of Binet and Courtier, but has more direct connection with actual musical composition.

Personal differences are so great and individual variation so uncertain that Mr. Sears finds it difficult to draw any general conclusions. It is clear that the notes do not follow the ratios represented by the written music. Accented notes, he finds, are lengthened, as has been noted by other investigators. There is a strong tendency to make the third note of a triplet the longest, and a slight tendency to lengthen the second over the first note.

Meumann's conjecture that one hand of a musician aids the other in preserving the time intervals is not borne out by a comparison of the tempo when only the air was played with than when both hands were used.

The method used in measuring the length of tones makes the results somewhat delusive. The records show that each tone usually began before the preceding tone ceased. Sometimes, however, there was an interval between the tones. In both cases Mr. Sears proceeded, as he says, 'on the assumption that each note was intended by the player to last until the following note was struck and no longer.' The overlap was therefore subtracted from the full time of the preceding note and the interval was added when it occurred. It is evident that under this method the durations of the tones shown in the tables was not the actual duration of the sounds. It would seem to be better to have measured the actual time which each key was held down. This would certainly have been nearer the actual psychical effect of the tones themselves.

Rhythm, Time and Number. ROBERT MACDOUGALL, New York University. Amer. Jour. of Psych., XIII., 88–97.

While this article is only critical and descriptive, it gains import from the knowledge that Professor MacDougall has recently carried on a lengthy empirical study of the rhythmic group which is to be published in the *Harvard Studies*.

Dr. MacDougall criticizes the statement of Sully that the accurate measurement of time intervals shows itself at its best in the perception of the rhythmic successions of verse and music. The opposite seems to be true from the experimental facts. An interval that is not part of a rhythmic succession is in general more accurately reproduced. The preservation of a rhythm depends not upon 'the accurate measurement of successive intervals of time * * * but the maintenance of proportionate relations among the successive groups.'

Another common belief, that motor accompaniment of regularly recurrent sensations tends to aid the estimation of their time values, is also combated by Dr. MacDougall. He claims that the process of motor adjustment either distracts the attention and thus injures the judgment, or else the material of judgment is transformed in value by being changed to agree with some rhythm of the organism.

Professor MacDougall agrees with those who make time estimation dependent on 'widespread tensions and releases in the organism. Moreover, he believes that this process cannot be 'localized in a single mechanism,' as Mach attempts to connect it with the ear.

In regard to number perception, attention is called to the experiment of Dietze that rhythmical integration is involved in the apprehension of serial impressions beyond about six members.

J. BURT MINER.

COLUMBIA UNIVERSITY.

EFFECTS OF ALCOHOL AND OF FASTING.

Ueber die Dauer der psychischen Alcoholwirkung. ERNST RUEDIN.

Ueber die Beeinflussung geistiger Leistungen durch Hungern. WILHELM WEYGANDT. Psychologische Arbeiten, Vierter Band, 1. Heft.

The entire number is given up to these two papers. The purpose of the first experiment was to make a further test on a number of individuals of the effect of a single dose of alcohol upon simple mental processes like choice reaction, associations, adding, and committing to memory. The experiments were made in the laboratory of Professor Kraepelin, and according to the methods well known to persons who have followed Professor Kraepelin's work. They were carried out upon three subjects for eight days and upon a fourth subject for eleven days. The effect of alcohol upon a single subject has been worked before so that the present study is an attempt to determine individual reactions to its effects and the duration of them. Tests were made in the morning at 9 o'clock, in the afternoon at 2:30 and in the evening at 9. The test of each mental process lasted for half an hour and five-minute pauses were allowed between the tests of the mental process. The order was association test, pause, adding, pause, and committing to memory. Only subject 4 took the choice reactions, and one hundred reactions constituted a test. The dose of alcohol was the amount contained in a half liter of Greek wine, 90 to 100 grams of absolute alcohol. It was taken by all subjects on the fourth

day of the experiments a half hour before the evening test. The evidence of the effect of the dose was found in the amount the regular practice increase from test to test was reduced in the tests following the drinking. The conclusions stated at the close of the paper are: (1) The effect of a large dose of alcohol of 90 to 100 grams upon four different persons shows great diversities in reference to direction, strength and duration. (2) The effect of alcohol consists in general in slower adding, difficulty of committing, shortening of choice time with increase of error reactions and finally in an increase of associations, especially those resting upon speech forms. Only with one subject was this last clear. (3) The duration of alcohol effects is generally from 12 to 24 hours, appearing in one case for 48 hours. The shortening of the choice time disappeared most quickly to give place then to a lengthening of the same with a continuation of error reactions. (4) The sensibility to alcohol is not alone dependent upon habituation to the poison, but it can be slight even after very long abstemiousness.

The second paper is devoted to a study of the influence exercised by fasting upon mental process. The method followed is somewhat similar to that followed in the alcohol research. Six reagents volunteered their services. All alcoholic and stimulating drinks were proscribed for several days before the experiments began and during the time they lasted. The tests were made upon the first day and this constituted the normal or control day for the series. On the second day all food was abstained from for twenty-four hours. In two series both food and drink were denied, and in one the time of fasting was lengthened to 48 hours and in another to 72 hours. The tests were continued for two days after the fasting to discover the duration of the effects, or after effects. With one reagent the experiments were continued for nine days, two days intervening between the periods of fasting, and with the other reagents the experiment lasted for only four days as indicated. The psycho-physical methods employed to test the effects of hunger were generally the space threshold for the cheek bone and the glabella, the time for 50 associations, the test of perception (Auffassungsfähigkeit) indicated by the reading time of 277 one-syllable words and 287 nonsense syllables pasted upon a revolving drum, the time for 200 choice reactions, continued associations for five minutes from a given word recorded by a stenographer, and a half hour devoted to learning nonsense syllables by heart. Not all reagents took the same tests in the same numbers or in the same order. The details with which the results are worked out are extremely complex and quite up to the standard of the German investigator.

The most important conclusions of the paper are: (1) Psychical deportment experiences a clear change during the abstinence from nourishment. (2) The effect is a sharply limited one in so far that some processes are much, others little and still others not at all, affected. (3) The capacity for perception or apprehension (Auffassungsfähigkeit) is not affected by hunger. (4) The conceivable connection in associative thinking is weakened; inner associations decrease; associations based upon use in speech increase; sound associations cease. The temporal flow of association is not changed. (5) Adding is measurably slowed. (6) The work of memorizing becomes continuously and clearly slower. This disturbance affects only attention and not the rapidity of speaking. (7) Choice reactions show a slight lengthening; the number of error reactions is somewhat increased in places. The effect of practice, mental fatigue, distraction, emotional excitability, are either only slightly or not at all affected by hunger. It is much to be regretted that so large a piece of work, covering as it does more than 130 pages, should not have yielded more definite results. Nothing new or very striking has come from either research communicated in this number, and they must be laid aside with some feeling of disappointment. They, however, emphasize the enormous difficulties that are encountered in work of this kind, and Professor Kraepelin is to be congratulated upon not having long since lost his patience and given up. The work is painstaking and careful and it is to be hoped that others will be incited to attack these same problems in different ways.

T. L. BOLTON.

UNIVERSITY OF NEBRASKA.

NEW BOOKS.

Development and Evolution. JAMES MARK BALDWIN. New York and London, The Macmillan Company. 1902. Pp. xvi + 395.

Elements of Experimental Phonetics. EDWARD WHEELER SCRIPTURE. New York and London, Charles Scribner's Sons. 1902. Pp. xvi + 627, and 26 plates.

Vorträge über Descendenztheorie. AUGUST WEISMANN. Jena, Fischer. 1902. Vol. I., pp. xii + 456; Vol. II., pp. vi + 462.

Die Grundsätze und das Wesen des Unendlichen in der Mathematik und Philosophie. KURT GESSLER. Leipzig, Teubner, 1902. Pp. viii + 417.

Experimental Sociology, Descriptive and Analytical. FRANCES A. KELLOR. New York and London, The Macmillan Company. 1901. Pp. xvi + 316.

Causeries Psychologiques. J. J. VAN BIERVLIET. Paris, Alcan.

NOTES.

DR. W. L. BRYAN has been elected President of Indiana University.

THE State University of Iowa has created a chair of psychology and elected to it Dr. C. E. Seashore, at present assistant professor in philosophy. Dr. Seashore took his doctor's degree at Yale in 1895, and was assistant in the Psychological Laboratory from 1895 to 1897.

W. S. JOHNSON, Ph.D. (Yale, '98), has been elected to the chair of philosophy and pedagogy in the University of Arkansas.

MR. J. H. BAIR, who last year held a scholarship in psychology at Columbia University, has been appointed assistant in anthropology. Mr. Elmer E. Jones, who also held a scholarship in psychology, has been elected professor of psychology and education in the State Normal School of Virginia.

A LABORATORY of experimental psychology will be opened next winter at King's College, London. It will be under the general supervision of the professor of physiology, Dr. Haliburton, and the special conduct will be entrusted to Dr. W. G. Smith, formerly of Smith College, Northampton, Mass.

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BALDWIN—Development and Evolution, including Psychophysical Evolution, Evolution by Orthopla-y, and the Theory of Genetic Modes. By JAMES MARK BALDWIN, Ph.D. Princeton, Hon. D.Sc. Oxon., LL.D. Glasgow, Stuart Professor in Princeton University.
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